PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXVI.

PART III.

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MDCCCXXVI.

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PHILOSOPHICAL

TRANSACTIONS.

XIV. *On the coagulation by heat of the fluid blood in an aneurismal tumour. By Sir* Everard Home, *Bart. V. P. R. S. Communicated January* 23, 1826.

Read March 2, 1826.

In a case of aneurism in the external iliac artery, in Chelsea Hospital, for the cure of which I tied the femoral artery below the sac, on the 16th September 1825; upon finding that this operation neither diminished the pulsation, nor arrested the increase of the size of the tumour, I was led to introduce a needle, to which is given the name of acu punctorium, into the centre of the tumour where the pulsation was most violent, and the fluid state of the blood most distinctly felt: the needle was passed through a small orifice in a bar of steel three inches long, the skin of the thigh was guarded by cork, and the needle was heated through the medium of the steel by a spirit lamp. In a few minutes the patient felt heat and pain in the centre of the tumour, but not very severe, so that the application was continued for fifteen minutes, during which the pulsation in the tumour was diminished; on withdrawing the needle the orifice was marked by a single drop of coloured serum. In half an hour intense pain was felt in the thigh; but this was not only removed in ten minutes by 20 drops of laudanum, but the thigh and leg became more easy than they had been for the previous twelve hours, and the throbbing in the sac was reduced to an undulation.

This application of heat was not resorted to till the 28th day after taking up the femoral artery, in which time the swelling had increased in size very considerably. The man was 43 years of age, had served 18 years in the 14th dragoon guards, and was a hard drinker. For two days the tumour was easy, and the pulsation had become less under the punctured part than higher up towards the belly. The tumour not diminishing, the operation was repeated on the 34th day from tying the artery; the needle and bar of steel double the size of those before used, and the application continued 35 minutes. The internal heat was greater than before, and the pulsation in the tumour much diminished. The needle was with difficulty withdrawn, a coagulum as hard as sealing-wax the size of a pin's head being firmly attached to the middle of the needle. The pain the operation produced subsided in ten minutes; the internal heat continued 24 hours, and the tumour had now a solid feel: he was quite easy for two days, but on the third the pain and pulsation returned, also the inward pain in a still greater degree than while the needle was immersed, and the tumour was extremely tense. Under these circumstances, on the 44th day after tying the femoral artery, I repeated the application of the acu punctorium with a needle and steel double the size of those last employed: the heat felt internally was very great, but the pain was not much increased: after it had been immersed 20 minutes the pulsation all at once stopped, and the needle was immediately withdrawn; the pain in ten minutes went off, and the patient was quite easy. From this time there was no pulsation in the tumour, which to the feel appeared solid, and therefore I considered the progress of the aneurism arrested; this was in some measure proved by the pulsation remaining violent in the external iliac artery down to the part pressed upon by the sac, but no further.

Although the patient was free from pain, took his usual allowance of food, and relished his wine and porter as much as before, the foot about the 50th day became cold to the touch, the thigh and leg œdematous, and about the 60th day vesications made their appearance on the foot and leg; the skin lost its sensibility, and although he had little or no pain, the weight of the thigh took from him all power of moving it.

These symptoms were the forerunners of mortification, the pressure from the tumour above preventing the leg from receiving a sufficient supply of nourishment; they did not however increase, and he remained in nearly the same state till the 90th day, when he died.

The upper part of the thigh, which had been regularly measured, was found on the 44th day to be 24 inches, and remained of the same size at his death, three inches and an half beyond that of the other.

On the examination of the parts after death, the external iliac artery was found to pass down in the front of the aneurismal sac; it was pervious in its course to the part where the ligature had been made on the femoral trunk; there it was plugged up by a small coagulum, beyond which it was impervious. The opening into the sac was on the posterior part, and was one inch in length. The coagulum formed by the heated needle had so unusual an appearance and texture, that it is shown in the annexed drawing. The upper portion of the sac occupied a part of the cavity of the pelvis, and by its pressure prevented the branches of the internal iliac artery from supplying the thigh with blood.

The coagulum in contact with the sac was the same as is usually met with in large aneurismal tumours; within that there were innumerable thin firm lamina, and the innermost portion was in the state of jelly.

It is a circumstance which could not have been foreseen, but one highly satisfactory to myself, that had I attempted to tie the artery above the formation of the aneurismal sac, I must have failed, since the sac extended higher than the division of the iliac artery into the external and internal, so that the external branch could not be secured without wounding the sac, which would have proved fatal; and in the space above, between the division into two iliacs, and the bifurcation of the aorta, there is an ossification an inch long; so that if the artery had been secured there, when the ligature separated, the patient must have bled to death. It may be said, that I might have taken up the aorta above the bifurcation; but I have made up my mind to let those diseases that require tying the aorta, which come under my care, take their course.

From the ossifications met with on the arterial trunks so much nearer the heart than the aneurismal sac, and a similar ossification having been formed in the femoral artery in the middle of the thigh, where it was taken up by Mr. HUNTER in the first instance in which he performed that operation for the cure of popliteal aneurism, we learn that in those individuals liable to aneurism, the larger trunks are more disposed to ossification than common, and therefore the tying such trunks is attended with no inconsiderable risk of hæmorrhage when the ligature comes away.

From the present case of aneurism, and Mr. HUNTER's, in which the success was complete, we find the contents of the sac are less readily absorbed than was believed. In proof of this, I have annexed a drawing of the natural size, taken from the remaining tumour of the popliteal artery of the patient on whom Mr. HUNTER first took up the femoral artery for the cure of aneurism in the ham, in the year 1785. The patient died of a fever 20 months after the operation, which afforded an opportunity of ascertaining the slow progress that had been made in the absorption of the contents of the aneurismal sac.*

From the statement that has been given, I trust that I have fully proved that the heated acu punctorium can coagulate the fluid blood contained in the aneurismal sac; and in doing so, is not productive of any local or constitutional symptoms that should prevent its being employed for that purpose.

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* Vide Trans. of a Society for Promoting Med. and Chirurg. Knowledge, vol. i. p. 138.

EXPLANATION OF THE PLATES.

Plate III. An anterior view of the aneurismal sac.

Fig. 1. A small portion of the aorta; the division into the two iliac arteries. The sub-division into the external and internal branches. The external iliac artery traced along its course to the opening into the sac. The upper part of the femoral artery slit open, from the lower orifice in the sac to the part where it became obliterated by the coming away of the ligature.

Fig. 2. The lower part of the aorta slit open; also the common trunk of the iliac artery, to show that the internal membranes of both have begun to ossify. The figures are of the natural size.

Plate IV. An internal view of the aneurismal sac.

Fig. 1. The aorta and external iliac passing on the outside of the aneurismal sac. The lamina of the coagulum first formed, and resembling those met with in other aneurismal tumours. The opening into the arterial trunk from the coats having given way for the space of one inch, exposing the upper and lower orifice of the artery; and showing that the sac adhered to the artery even higher than the going off of the internal iliac.

Fig. 2. A small portion of the blood coagulated by the heated needle, which has a foliated structure, the leaves thin, compact, and connected together by thread-like filaments. Both these figures of the natural size.

Fig. 3. One of these leaves magnified ten diameters; its surface studded over with nodules of coagulable lymph, like so many drops of melted wax.

Fig: 1.

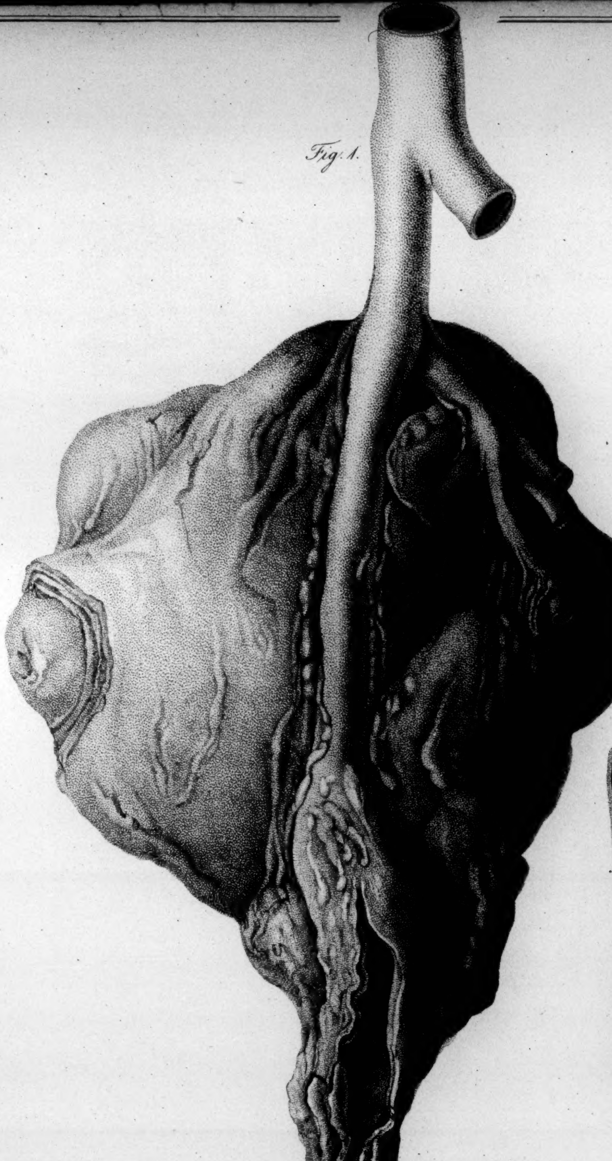

Fig. 2.

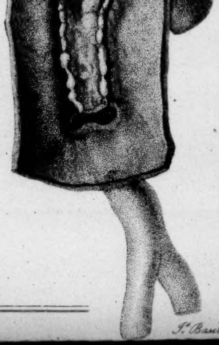

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Fig. 3.

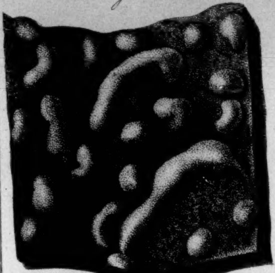

Fig. 2.

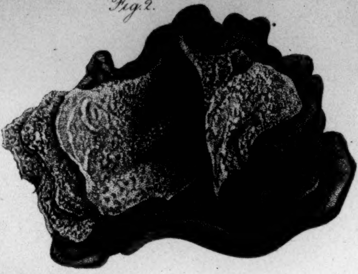

Fig. 1.

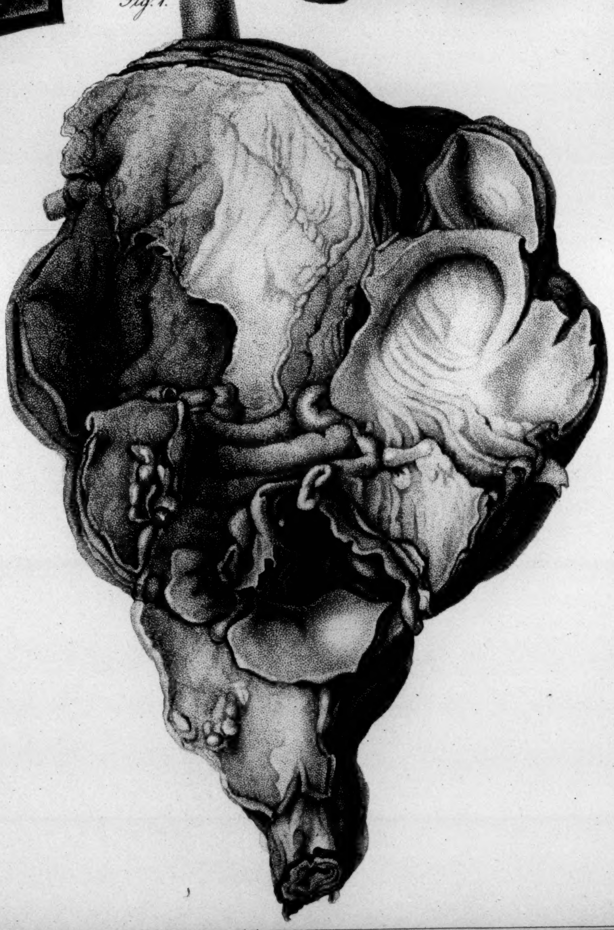

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Fig. 1.

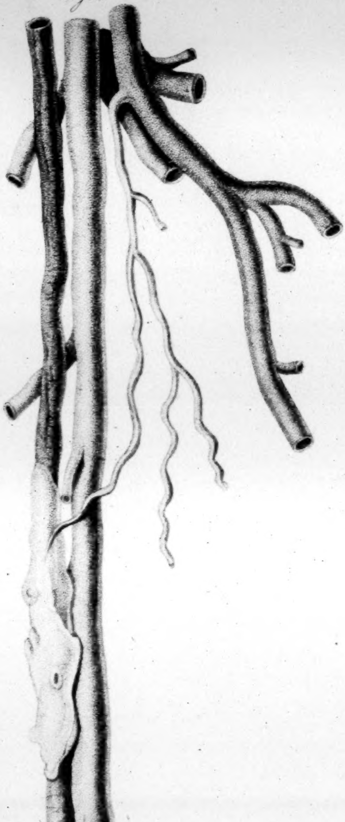

Fig. 2.

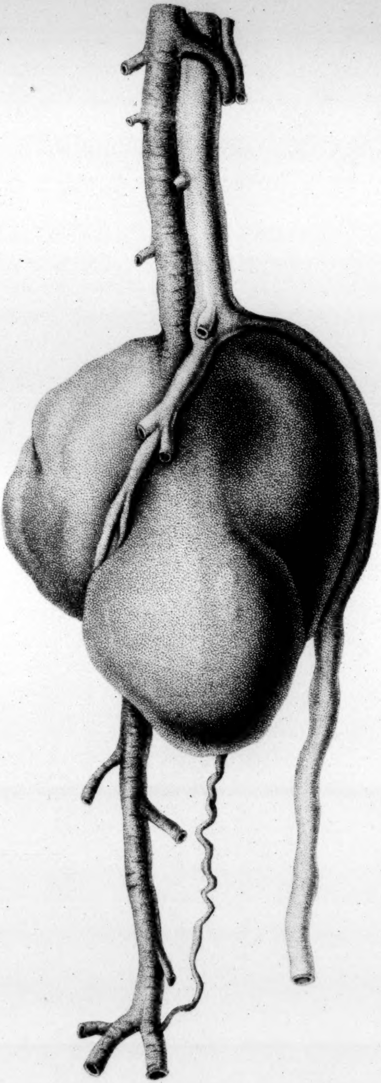

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Plate V.

A representation of the appearance of the remains of the tumour twenty months after the femoral artery had been tied by Mr. Hunter, in the first case in which he performed that operation for the cure of popliteal aneurism, in the year 1785, at St. George's Hospital. The case was published in 1793, in the first volume of the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, but no engraving accompanied that account. That now given is taken from the parts removed from the body after death, which are rendered distinct by having been injected.

Fig. 1. Shows the femoral artery, the canal of which had been rendered impervious from the going off of the large muscular branch called profunda, down to where the ligature had been tied, and there it was ossified. Below this it retained its natural size. There is a small branch from the arteria profunda anastomosing with the femoral trunk immediately below the impervious portion, at the part where the coats afterwards became ossified. The femoral vein accompanies the artery.

Fig. 2. represents the continuation of the femoral artery, showing the dilatation into the aneurismal sac; the size of which is much greater than could have been expected had the absorption gone on with the same activity as in naturally formed parts, 20 months, after the disease had been put a stop to.

Below the aneurismal tumour is a large anastomosing branch from the profunda, with the trunk of the popliteal artery before its division. The veins are spread over the tumour.

On the effect of heated iron upon fluid blood out of the body.

That I might ascertain the effect of heated iron immediately after it was applied to fluid blood, I bored a hole in the centre of the bottom of a horn cup, through which the same acu punctorium used in the case of aneurism was passed up into the middle of the vessel; and when it was heated by the same bar of iron and spirit lamp, three ounces of blood were received from the arm into the cup, and the heat continued for half an hour, after which the blood was left at rest for 24 hours.

The coagulum had no buff on the surface. When a perpendicular section was made of it, the mass of the red coagulum was interspersed with minute white particles of coagulable lymph, and that part in contact with the needle, for the space of half an inch round, was in the same state as is represented in the drawing of the aneurismal coagulum. To carry on this investigation with pieces of iron, the heat of which was ascertained, I made the following experiment: three pieces of iron, weighing an ounce each, with iron handles covered with wood, were placed in separate cups, and blood from the arm immediately received into them to the amount of three ounces. In the first, the iron was red hot, in the second, the red colour had just gone off, and the third was taken out of boiling water. In five minutes the heat of each portion of blood was ascertained by the thermometer, the pieces of iron being removed. No. 1. was 110°, No. 2. 105°, No. 3. 85°, the atmosphere 55°. After standing 24 hours, perpendicular sections were made of them.

No. 1. had separated seven drams of serum, had a greyish film $\frac{1}{40}$th of an inch thick; the coagulum was coloured, loose in its texture, interspersed with specks of white coagulable lymph; and at the lower part there was a cell, the shape of the piece of iron, the sides of which had the same frosted floculent appearance seen in the drawing of the aneurism. No. 2. had separated 12 drams of serum, had a film $\frac{1}{20}$ of an inch thick, the coagulum was more solid, coloured, interspersed with specks of coagulable lymph, and had a cell in all respects like that in No. 1.

No. 3. had separated 12 drams of serum, had a buffy coat, much cupped, $\frac{1}{8}$ of an inch thick; the coloured coagulum very solid, and no cavity was found where the iron had been situated.

To determine the effect of lower temperature, I made a similar experiment with three leaden ounce balls, 1st heated to 200°, 2d to 160°, 3d to 120°. At the end of 24 hours, the balls having been left in the cups, No. 1. had a film of buff $\frac{1}{20}$th part of an inch, separated 4 drams of serum, round the cavity surrounding the ball a film $\frac{1}{40}$th of an inch. No. 2. had a film $\frac{1}{24}$th of an inch, serum $6\frac{3}{4}$ drams, round the ball no film. No. 3. buffy and cupped, the buff $\frac{1}{16}$th of an inch, whiter and tougher than the other. The overplus blood drawn into a porringer not in the least buffy.

From these experiments, blood is rendered buffy at the top by a heat of 120°, and when poured on substances heated above 212° the coagulable lymph is separated where it comes in contact with the heated body.

It is a fact ascertained, that when blood is drawn from the arm into a cup immersed in boiling water, which is kept for some hours at that heat, does not form a complete coagulum; but if drawn into a cup immersed in ice and left there, in 24 hours I found the surface had a buffy coat $\frac{1}{8}$ of an inch thick; and although 5 drams of serum had separated, the coagulum was so loose as hardly to hold together.

A new fact respecting buffy blood after venesection.

It has been generally believed that the cupped appearance met with on the surface of the coagulation of blood drawn from the arm, arises from the coagulable lymph, when separated from the other parts of the blood, having a greater contractile power than when intimately mixed with them.

In a patient who had an affection of the brain in consequence of an injury to the skull from a wooden shovel, when 35 years of age, no symptoms came on for 8 months. He then had fits, and head-ache, which nothing relieved : he was bled to the amount of 68 oz. in 13 days ; but the last two cups, although equally buffy with all the others, were not cupped, which took my attention. The appearance is shown in the annexed drawing, proving that the coagulable lymph has less power of contraction than the other parts of the blood.

The malady was found, by examining the body after death, to be inflammation of one of the convolutions of the right hemisphere of the cerebrum.

In the microscope, the upper portion without colour consisted wholly of globules $\frac{1}{2,800}$ and $\frac{1}{3,200}$ parts of an inch in diameter ; the lower coloured portion was entirely made up of globules $\frac{1}{2,000}$ and $\frac{1}{2,400}$ parts of an inch in diameter.

Professor Brande, who examined the chemical properties of these different portions, informs me, " that the upper portion had the usual properties of the albumen of the blood. " When carefully dried, it assumed the appearance and che-

"mical characters of horn. The lower coloured portion was "looser in texture and considerably softer. In dilute acetic "acid it soon became perfectly gelatinous, and was dissolved "in it by the aid of a gentle heat. It contained a much "larger relative proportion of water than the upper portion, "and shrunk much more when dried, in which state, however, "it resembled dried albumen. It appears that independent "of colouring matter, these two portions of coagulum differ "chiefly from each other in their state of concentration, the "upper part containing more albumen, and less water, than "the lower."

EXPLANATION OF PLATE VI.

Fig. 1. The coagulum represented entire. While immersed in the serum the surface was flat, but after remaining a few hours in weak spirit to prevent it from falling to pieces, the surface put on its present appearance.

Fig. 2. A perpendicular section of the same coagulum. Both of these figures are of the natural size.

Fig. 3. A small part of the upper portion of fig. 1. magnified 200 diameters, to show the size of the globules of which it was composed to be from $\frac{1}{2800}$ to $\frac{1}{3,200}$ parts of an inch in diameter.

Fig. 4. A small part of the lower portion, fig. 2. magnified 200 diameters; showing the size of the globules it contained to be from $\frac{1}{2,000}$ to $\frac{1}{2,420}$ parts of an inch in diameter, mixed with a coloured gelatinous substance.

Fig. 5. A coagulum of blood taken from the arm of a person labouring under symptoms of inflammation of the

Fig. 1.

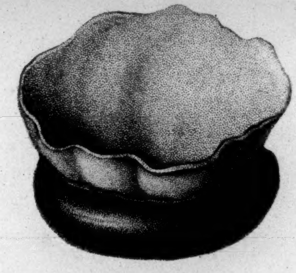


Fig. 2.

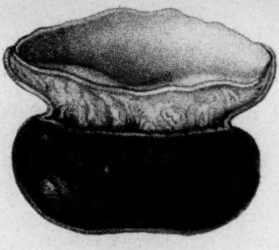
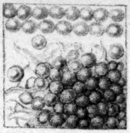
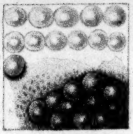


Fig. 3.



From 2800 to 3200 parts of an inch.

Fig. 4.



From 2000 to 2400 parts of an inch.

Fig. 5.

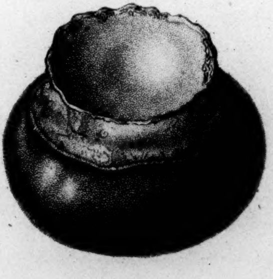
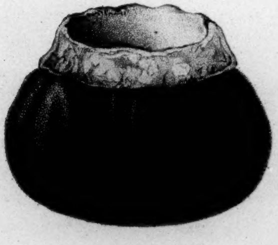


Fig. 6.



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lungs; the upper surface is concave; and the coagulable lymph of which it is composed is not entirely free from an admixture of the other component parts of the blood.

Fig. 6. A perpendicular section of fig. 5. The two last figures are represented of their natural size.

XV. *On the mathematical theory of suspension bridges, with tables for facilitating their construction. By* Davies Gilbert, *Esq. V.P.R.S. &c. Communicated March* 9, 1826.

Read March 9, 1826.

My attention was first directed to a consideration of suspension bridges, and of the catenary curve on which their theory depends, when the plan for making such a communication across the Menai Straits was submitted to the Commissioners appointed by Parliament to improve the communication by roads and bridges through Wales. It then appeared to me, that the proposed depth of curvature, was not sufficient for ensuring such a degree of strength and permanence as would be consistent with the due execution of a great national work. This opinion I advanced as a Member of the Commission. But wishing to take on myself the full responsibility for such increased expense, as must of necessity be occasioned by enlarging the curvature, I also printed some approximations, hastily deduced, in the Quarterly Journal of Science; and derived from them a confirmation of the opinion that had been given. The interval between the points of support and the road-way of the Menai Bridge has in consequence been augmented to fifty feet; and it now possesses that full measure of strength, which experience has established as requisite and sufficient for works of iron not perfectly at rest.

Since bridges of suspension are obviously adapted to very general use, I have flattered myself with the hope of doing something serviceable to the public, by expanding into tables the formulæ from which my approximations were derived; adding to them other formulæ and tables for the catenary of equal strength. A curve not merely of speculative curiosity, but of practical use, where a wide horizontal extent may chance to be combined with natural facilities for obtaining a correspondent height for the attachments.

Both the ordinary catenaries, and these of equal strength, like circles, parabolas, logarithmic curves, &c. have the property of being each identical with themselves in every respect but size: and as the radius, the parameter, and the subtangent give the respective magnitudes of these curves, so are the catenaries determined in magnitude by the tension (expressed in measures of the chain), which takes place at the middle point, or apex of the curve, where it is a minimum. Consequently, when this tension is determined or given, all the other relations may be expressed in the same manner as sines, cosines, &c. in the circle.

I assume that the first principles of the catenary curve are known; they will, consequently, be noted with no other view, than to derive from them ulterior properties.

For the ordinary catenary:

Let $a =$ the tension at the apex, estimated in measures of the chain;

$x =$ the absciss, the versed sine, or depth of curvature;

$y =$ the ordinate, or semi-transverse length;

$z =$ the length of the curve.

Then since the tension, $(a,)$ acts horizontally at the apex (A,) since the weight of the chain (z) acts at right angles to the former, and the force of suspension at (P) acts in the direction of the tangent. These forces must be represented in direction and in magnitude by the incremental triangle Prp; and

As $\dot{x} : \dot{y} :: z : a$; as $\dot{x}^2 : \dot{y}^2 :: z^2 : a^2$;

as $\dot{x}^2 + \dot{y}^2 : \dot{x}^2 :: a^2 + z^2 : z^2$;

But $\dot{x}^2 + \dot{y}^2 = \dot{z}^2$ universally.

Therefore, $\dot{z}^2 : \dot{x}^2 :: a^2 + z^2 : z^2$; and $\dot{x} = \frac{z\dot{z}}{\sqrt{a^2 + z^2}}$; consequently, $x = \sqrt{a^2 + z^2} - a$.

$$\text{Equation A} \begin{cases} \text{No. 1. } x = \sqrt{a^2 + z^2} - a \\ \text{No. 2. } z = \sqrt{2ax + x^2} \\ \text{No. 3. } a = \frac{z^2 - x^2}{2x} \end{cases}$$

Again; $\dot{x} : \dot{y} :: z : a \therefore \dot{y} = \frac{a\dot{x}}{z}$, substituting from Eq. A. (No. 2.

$\dot{y} = \frac{a\dot{x}}{\sqrt{2ax + x^2}}$; and

Equ. B. No. 1.) $y = a \times$ natural log. of $\frac{a + x + \sqrt{2ax + x^2}}{a} =$ $a \times$ nat. log. $\frac{a + x + z}{a}$; or by substituting its value for a from Equ. A. No. 3, and dividing by $z + x$.

Equ. B. No. 2.) $y = a \times$ nat. log. $\frac{z + x}{z - x}$;

or, if $\frac{z\dot{z}}{\sqrt{a^2 + z^2}}$ be substituted for $\dot{x}$ in

$\dot{y} = \frac{a\dot{x}}{z}$. $\dot{y} = a \times \frac{\dot{z}}{\sqrt{a^2 + z^2}}$ and

Equ. B. No. 3.) $y = a \times$ nat. log. $\frac{\sqrt{a^2 + z^2} + z}{a}$.

To find x when a and y are given:

Let N = the number of which $\frac{y}{a}$ (Equ. B. No. 1.) is the natural logarithm.

Then $aN = a + x + \sqrt{2ax + x^2}$, and $\sqrt{2ax + x^2} = aN - a - x$, make $aN - a = M$. Then $2ax + x^2 = M^2 - 2Mx + x^2$, and

Equ. C.) $x = \frac{M^2}{2M + 2a}$,

x being known.

z is found from Equ. A. No. 2. and

T, the tension at P, being obviously equal to $\sqrt{a^2 + z^2}$, is equal (Eq. A. No. 2.) to $\sqrt{a^2 + 2ax + x^2} = a + x$.

The angle of suspension is derived from the common analogy of the incremental triangle, and of the forces corresponding with it.

Tables I. and II. are constructed from these theorems, and their use will be best explained by an example.

Let the span proposed for a suspension bridge be 800 feet, and let the adjunct weight of suspension rods, road-way, &c. be taken at one-half of the weight of the chains; then, if the full tenacity of iron is represented by the modulus of 14800 feet, the virtual modulus for the whole weight must be reduced in the proportion of $2 + 1 : 2$, or to 9867 feet; and let it be determined to load the chains at the point of their greatest strain, that is at the points of suspensions, with one-sixth part of the weight they are theoretically capable of sustaining.

Then, since the semi-span is 400 feet, and y in Table I. is taken at an hundred measures, each of these measures must be four feet, and the weight expressed in the same measures to be sustained at the points of suspension will be $9867 \div 6 \times 4 = 411{,}125$. Now it appears from Table I. where y is uniformly an hundred, that when $T = 412$

$a = 400$ measures or 1600 feet.
$x = 12.565$ - - 50.260
$z = 101.045$ - 404.180
$<$ the angle of suspension 75° 49′.

Having now determined a, the modulus, latus rectum, or parameter of the curve. In Table II. will be found all the respective quantities for each measure of y. But as a is in this table taken at an hundred measures, and it has been found to be 400 of the former, each measure here must be 4 times 4, or 16 feet; consequently, each gradation of y will also be 16 feet, and the whole semi-span $\frac{400}{16}$ or 25 measures. And since z will be given in the Table for each measure of y, the adjunct weights may readily be adapted to a strict preservation of the catenary form.

$$\begin{array}{r} \text{At 21 measures of } y \,.\, z = 21.1547 \\ \text{20 measures of } y \,.\, z = 20.1335 \\ \hline 1.0212 \times 16 = 16.3392 \text{ feet.} \end{array}$$

Consequently while the ordinate extends one measure, or 16 feet from the 20th to the 21st measure, the length of the curve will increase 16 feet and $\frac{1}{3}$ very nearly, and the adjunct weight should be increased in the same proportion.

At 21 the length of x is 2,2131 measures, or multiplied by 16 = 35,4096 feet, the length of the suspension rods to the level of the apex.

It appears from Table I. that the tension T for a given half span of 100 measures is very nearly at its minimum when $x = 65{,}85$ measures, almost one-third part of the whole span. In the example taken above $65.85 \times 4 = 263{,}4$ feet, an height not to be attained in practice, nor strictly upplicable if it could be reached, because of the great length of suspension. If the span and height ($2\,y$ and x) were given, the other quantities would be found in a similar manner.

In the catenary of equal strength

$a \,.\, x \,.\, y \,.\, z$ remain as before; but another symbol must now be introduced, ζ = the mass of the chain. Then will the forces be represented as in the ordinary curve by the incremental triangle P*rp*. But now $\dot{x} : \dot{y} :: \zeta : a$. And by a repetition of the former steps $\dot{x} = \frac{\zeta \dot{z}}{\sqrt{a^2 + \zeta^2}}$.

But on the principle of equal strength,

As $a : \sqrt{a^2 + \zeta^2} :: \dot{z} : \dot{\zeta}$

therefore $\dot{z} = a \times \frac{\dot{\zeta}}{\sqrt{a^2 + \zeta^2}}$ and

Equ. D.) $z = a \times \text{nat. log.} \frac{\sqrt{a^2 + \zeta^2} + \zeta}{a}$;

and by substituting $a \times \frac{\dot{\zeta}}{\sqrt{a^2 + \zeta^2}}$ for $\dot{z}$ in the equation $\dot{x} = \frac{\zeta \dot{z}}{\sqrt{a^2 + \zeta^2}}$

$\dot{x} = a \times \frac{\zeta \dot{\zeta}}{a^2 + \zeta^2}$; consequently,

Equ. E.) $x = \frac{a}{2} \times \text{nat. log.} \frac{a^2 + \zeta^2}{a^2}$.

Again, from the first analogy, $\dot{y} = \frac{a \dot{x}}{\zeta}$,

substitute for $\dot{x}$ its equal $a \times \frac{\zeta \dot{\zeta}}{a^2 + \zeta^2}$, and

$\dot{y} = a^2 \times \frac{\dot{\zeta}}{a^2 + \zeta^2}$; therefore,

Equ. F.) $y =$ the cir. arc. of which ζ is the tangent to radius a.

a and y being given to find ζ. Multiply $\frac{y}{a}$ by 57°,29578 (the tab. log. 1.7581226) and reduce the decimals of a degree into minutes and seconds; then will the tangent of that arc, multiplied by a, be equal to ζ.

And when ζ has been determined, the other columns of Tables III. and IV. are constructed from the above theorems, in a manner perfectly similar to that used in calculating of Tables I. and II.; and they may be illustrated by the same example; observing that a, now represents the uniform tension on each given magnitude of iron throughout the chains, and that the column T has the whole pull which any building or support may have to sustain in the direction of the tangent.

In Table III. y being, as before, an hundred measures of four feet each, a must be sought $=411{,}125$, and by proportioning between 420 and 400

$$\left.\begin{array}{l} x = \ 12.2904 \\ z = 101.0020 \\ \zeta = 102.0235 \\ T = 423.6019 \end{array}\right\} \begin{array}{c} \text{- - - -} \\ \text{- - - -} \\ \text{measures or} \\ \text{- - - -} \\ \text{- - - -} \end{array} \left.\begin{array}{r} 49.1616 \\ 404.0080 \\ 408.0940 \\ 1694.4076 \end{array}\right\} \text{Feet.}$$

$< .\ 76^\circ\ 3'\ 17''.$

a, or the modulus of this curve being fixed at 411,125 measures of 4 feet each, or at 1644,5 feet; and a in Table IV. being taken at 100 measures, each one will be 16,445 feet, and all the quantities are given for each gradation of y.

Thus at 21 measures of $y\,.\ z = 21.1564\ \ \zeta = 21.3142$

20 measures of $y\,.\ z = 20.1347\ \ \zeta = 20.2710$

$\ 1{,}0217\ \ \ 1.04332$

$1.0217 \times 16.445 = 16.8019$ feet the increase of z;

$1.0432 \times 16.445 = 17.1410$ feet the increase of material in ζ: consequently $\frac{1.0432}{1.0217} = 1.021$, the quantity of matter in this part of the chain to maintain uniform strength, that at the apex being unity, and the adjunct matter should be in the proportion of 1 to 1,04332.

Moreover x the versed sine, or the length of the suspension rods to the level of the apex will be at

21 measures of $y\,.\ x = 2.2214$ measures $\times\ 16{,}445 = 36{,}531$ ft.

20 measures of $y\,.\ x = 2.0135$ measures $\times\ 16.445 = 33.112$ ft.

Assuming in the ordinary catenary that $x = 65.85$ measures, is the height of the attachment to give a maximum extent of span with any virtual tenacity of material, a will be 85 measures, and $a + x = 85 + 65.85$, or 150,85 measures equal the given virtual tenacity. This taken as before at $\frac{2}{3}$ of $\frac{1}{6}$ of 14800 feet, will give 10,875 feet for each measure, and the whole span at $2y = 2175$ feet. Chains merely supporting themselves, and at the utmost of their tenacity will extend nine times further, or to 19575 feet.

In the catenary of equal strength, the semi-span being equal to the circular arc of which ζ is the tangent to radius a, it is obvious that $a \times$ semi-cir. arc must be the limit of the span. Therefore if $a = \frac{2}{3}$ of $\frac{1}{6}$ of 14800 feet, or 1644,44 $a \times \frac{c}{2} = 5154$ feet.

And if the chains merely sustain themselves at their utmost tenacity, 5154×9 will give 46385 feet, equal to 8,785 miles, or somewhat more than 8 miles and three-quarters.

But this case is purely hypothetical, for the purpose of ascertaining a limit, since ζ, the mass or weight of the chain must be infinite, and consequently its length: the figure approaching indefinitely near to that of a chain sustaining itself from an infinite height, which figure is identical with that of a building, capable so far as pressure and the strength of materials are alone concerned, of being carried to any elevation whatever. This figure is readily determined:

Let $a =$ the section of such a building at its base,
$y =$ the section at any height,
$x =$ that height;

Then, since the section and the superincumbent pressure must always be in the same proportion to each other, $\dot{x}$ and $\frac{\dot{y}}{y}$ are in a constant ratio. Let then $\frac{\dot{x}}{m} = -\frac{\dot{y}}{y}$ where m is the modulus of pressure in the given material; but when $x = o, y = a$, therefore $\frac{x}{m} =$ the nat. log. $\frac{a}{y}$; or $\frac{x}{A.m} =$ the tab. log. $\frac{a}{y}$. $A = 2{,}3025851$; but if ε and γ the homologous sides or diameters of these sections; then, $\frac{x}{2.A.m} =$ tab. log. $\frac{\varepsilon}{\gamma}$.

Finally, I would notice a correction of frequent use in practical surveying, to be deduced from the properties of the catenary curve.

When the measuring chain is extended over ground uneven, intersected by ditches, or made soft by water, it cannot be laid flat, but must be elevated at both its extremities, while the middle just touches the surface: thus giving the measurement too great by the difference between the whole perifery and the double ordinate.

Let $z =$ the half length of the chain.

$x =$ the elevation at each end equal to the depths of curvature.

Then Equ. B. No. 2. $y = a \times$ nat. log. $\frac{z+x}{z-x}$,

And Equ. A. No. 3. $a = \frac{z^2-x^2}{2x}$; therefore

$$y = \frac{z^2-x^2}{2x} \times \text{nat. log.} \frac{z+x}{z-x}.$$

But when x is very small in comparison of z, the nat. log. of $\frac{z+x}{z-x}$ becomes $\frac{2x}{z}$, and

$$y = \frac{z^2-x^2}{2x} \times \frac{2x}{z} = z - \frac{x^2}{z};$$

$\frac{x^2}{z}$ is therefore the difference between half the chain and the ordinate. If x be expressed in parts of the whole chain, $4\,x^2$ will be the correction for the difference between the perifery and double ordinate.

If x (the elevation at each end) be one link of the common measuring chain, $4\,x^2 = \frac{1}{25}$ of a link, $\frac{1}{25}$ of $\frac{66}{100}$ of a foot $= 0.3168$ of an inch, varying as the squares of x.

If half the chain were considered as a straight line, and the hypothenuse of a right angled triangle, the horizontal distance would be $z - \frac{x^2}{2z}$, giving but one half of the true difference, 0,1584 parts of an inch.

And if the chain were supposed to be in the arc of a circle, $z = y + \frac{y^3}{6az}$, &c. And $y = \sqrt{2ax - x^2}$ (when x is very small in comparison with a) $= \sqrt{2ax}$. Therefore $a = \frac{y^2}{2x}$.

And since y is also small in comparison of a, the second term of the series $\left(\frac{y^3}{6a^2}\right)$ will be the difference between the ordinate and the arc. Then substituting $\frac{y^4}{4x^2}$ for a^2, $\frac{y^3}{6a^2} = \frac{2x^2}{3y}$; or if x be expressed in parts of the whole chain, $= \frac{8}{3}\,x^2$ will be the whole correction, $= 0.2112$ parts of an inch, or two-thirds of the true difference.

Formulæ might readily be constructed for different elevations of the extremities of the chain, but they would prove much too complicated for practical use.

One further observation may be applicable to suspension bridges, wholly unconnected with the preceding investigations.

In the event of their wanting stability to counteract and restrain undulatory motion, the ballustrades may be carried to any required height, and rendered inflexible by diagonal braces; and if further means were required for imparting stability, such braces might be adjusted with screws to the suspension rods themselves, after these rods had acquired their exact positions, on the completion of the work.

Table I.— Ordinary Catenary.

$y = 100.$

a.	N.	*x.*	*z.*	T.	Angle.
2000	1.051271	2.500511	100.041474	2002.500511	87° 8 11″
1950	1.052619	2.564593	100.042440	1952.564593	87 3 46
1900	1.054041	2.632163	100.045727	1902.632163	86 59 8
1850	1.055541	2.703298	100.047540	1852.703298	86 54 15
1800	1.057127	2.778421	100.050163	1802.778421	86 49 6
1750	1.058807	2.857914	100.054318	1752.857914	86 43 40
1700	1.060588	2.942018	100.057566	1702.942018	86 37 53
1650	1.062480	3.031204	100.060788	1653.031204	86 31 46
1600	1.064494	3.125974	100.064421	1603.125974	86 25 16
1550	1.066642	3.226852	100.068245	1553.226852	86 18 21
1500	1.068939	3.334558	100.073939	1503.334558	86 10 59
1450	1.071399	3.449618	100.078929	1453.449618	86 3 6
1400	1.074041	3.572907	100.084490	1403.572907	85 54 39
1350	1.076886	3.705344	100.090750	1353.705344	85 45 35
1300	1.079958	3.847958	100.097440	1303.847958	85 35 45
1250	1.083286	4.002035	100.105463	1254.002035	85 25 16
1200	1.086903	4.168981	100.114680	1204.168981	85 13 51
1150	1.090849	4.350543	100.125801	1154.350543	85 1 26
1100	1.095169	4.548545	100.137346	1104.548545	84 47 54
1050	1.099920	4.765440	100.150553	1054.765440	84 33 5
1000	1.105170	5.004084	100.165906	1005.004084	84 16 48

The column in Table I. marked N (where the numbers equal $e^{\frac{y}{a}}$) is given as the medium conducting to all the subsequent calculations.

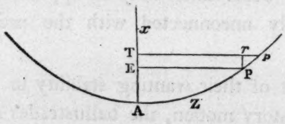

Table I. *continued.*—The Ordinary Catenary.

$y = 100$.

a.	N.	*x.*	*z.*	T.	Angle.
1000	1.105170	5.004084	100.165906	1005.004084	84° 16′ 48″
980	1.107428	5.106408	100.173025	985.106408	84 9 49
960	1.109785	5.213007	100.180582	965.213007	84 2 13
940	1.112247	5.324098	100.188974	945.324098	83 54 58
920	1.114822	5.440045	100.196191	925.440045	83 47 4
900	1.117519	5.561266	100.205825	905.561266	83 38 48
880	1.120344	5.687876	100.214837	885.687876	83 30 11
860	1.123309	5.820479	100.225255	865.820479	83 21 9
840	1.126423	5.959364	100.235949	845.959364	83 11 42
820	1.129698	6.105033	100.247321	826.105033	83 1 47
800	1.133148	6.258102	100.260296	806.258102	82 51 23
780	1.136785	6.418938	100.273356	786.418938	82 40 28
760	1.140627	6.588360	100.288153	766.588360	82 28 57
740	1.144691	6.767004	100.304328	746.767004	82 16 50
720	1.148996	6.955577	100.321527	726.955577	82 4 3
700	1.153564	7.154926	100.339869	707.154926	81 50 33
680	1.158422	7.366193	100.360765	687.366193	81 36 15
660	1.163595	7.590181	100.382517	667.590181	81 21 6
640	1.169118	7.828368	100.407143	647.828368	81 5 1
620	1.175025	8.081923	100.433570	628.081923	80 47 54
600	1.181360	8 352608	100 463404	608.352608	80 29 40
580	1.188169	8.642033	100.495985	588.642033	80 10 11
560	1.195508	8.952299	100.532176	568.952299	79 49 27
540	1.203419	9.283888	100.562366	549.283888	79 27 2
520	1.212043	9.645021	100.617335	529.645021	79 2 56
500	1.221402	10.033315	100.667683	510.033315	78 36 59
480	1.231625	10.454508	160.725490	490.454508	78 8 55
460	1.242830	10.912412	100.789382	470.912412	77 38 28
440	1.255172	11.412622	100.863052	451.412622	77 5 23
420	1.268829	11.961025	100.947150	431.961025	76 29 6
400	1.284025	12.565207	101.044792	412.565207	75 49 22
380	1.301032	13.233994	101.158163	393.233994	75 5 35
360	1.320192	13.978365	101.290757	373.978365	74 17 7
340	1.341941	14.812141	101.447796	354.812141	73 32 10
320	1.366837	15.752501	101.635337	335.752501	72 22 46
300	1.395612	16.821529	101.862069	316.821529	71 14 44
280	1.429239	18.047685	102.139232	298.047685.	69 57 31
260	1.469049	19.468993	102.483745	279.468993	68 29 13
240	1.516896	21.126437	102.893226	261.126437	66 47 38
220	1.575420	23.118850	103.473548	243.118850	64 48 38
200	1.648721	25.525175	104.219022	225.525175	62 28 34
180	1.743908	28.559946	105.343499	208.559946	59 39 43
160	1.868245	32.280531	106.638654	192.280531	56 19 0
140	2.042722	37.258541	108.722538	177.258541	52 10 2
120	2.300975	44.134402	111.982596	164.134402	46 58 48
100	2.718281	54.308027	117.520071	154.308027	40 23 42
95	2.865180	57.674415	119.517684	152.674415	38 28 45
90	3.037731	61.511583	121.884206	151.511583	36 26 34
85	3.240907	65.852160	124.624934	150.852160	34 17 44
80	3.490342	71.073875	128.153485	151.073875	31 58 28
75	3.793667	77.147407	132.377616	152.147407	29 32 4
70	4.172733	84.433443	137.657866	154.433443	26 57 10

Table II.—The Ordinary Catenary.

$a = 100.$

N.	*y.*	*x.*	*z.*	T.	Angle.
					° ′ ″
1.010050	1	.004999	1.000000	100.004999	89 25 39
1.020201	2	.020000	2.000100	100.020000	88 51 15
1.030454	3	.045001	3.000398	100.045001	88 16 53
1.040810	4	.080007	4.000992	100.080007	87 42 31
1.051271	5	.125025	5.002074	100.125025	87 8 11
1.061836	6	.180050	6.003540	100.180050	86 33 51
1.072508	7	.245098	7.005701	100.245098	85 59 33
1.083287	8	.320170	8.008520	100.320170	85 25 16
1.094174	9	.405271	9.012128	100.405271	84 51 1
1.105170	10	.500408	10.016591	100.500408	84 16 48
1.116278	11	.605609	11.022190	100.605609	83 42 36
1.127496	12	.720855	12.028744	100.720855	83 8 37
1.138828	13	.846186	13.036613	100.846186	82 34 20
1.150273	14	.981591	14.045708	100.981591	82 0 14
1.161834	15	1.127107	15.056292	101.127107	81 26 15
1.173510	16	1.282710	16.068289	101.282710	80 52 17
1.185304	17	1.448471	17.081928	101.448471	80 18 22
1.197217	18	1.624373	18.097326	101.624373	79 44 31
1.209249	19	1.810427	19.114472	101.810427	79 10 43
1.221402	20	2.006663	20.133536	102.006663	78 36 59
1.233678	21	2.213114	21.154685	102.213114	78 3 19
1.246076	22	2.429763	22.177836	102.429763	77 29 43
1.258600	23	2.656680	23.203319	102.656680	76 56 11
1.271249	24	2.893847	24.231042	102.893847	76 22 45
1.284025	25	3.141302	25.261197	103.141302	75 49 22
1.296929	26	3.399061	26.293838	103.399061	75 16 5
1.309964	27	3.667187	27.329212	103.667187	74 42 53
1.323129	28	3.945662	28.367237	103.945662	74 9 46
1.336427	29	4.234542	29.408157	104.234542	73 36 44
1.349858	30	4.533833	30.451966	104.533833	73 3 48
1.363424	31	4.843577	31.498822	104.843577	72 30 58
1.377127	32	5.163822	32.548877	105.163822	71 58 13
1.390968	33	5.494589	33.602210	105.494589	71 25 35
1.404947	34	5.835881	34.658818	105.835881	70 53 3
1.419067	35	6.187768	35.718931	106.187768	70 20 36
1.433329	36	6.550276	36.782623	106.550276	69 48 18
1.447734	37	6.923431	37.849968	106.923431	69 16 6
1.462284	38	7.307284	38.921115	107.307284	68 44 0
1.476980	39	7.701863	39.996336	107.701863	68 12 1
1.491824	40	8.107217	41.075182	108.107217	67 40 10
1.506817	41	8.523379	42.158320	108.523379	67 8 25
1.521961	42	8.950402	43.245697	108.950402	66 36 48
1.537257	43	9.388315	44.337384	109.388315	66 5 19
1.552706	44	9.837146	45.433453	109.837146	65 33 57
1.568312	45	10.297011	46.534188	110.297011	65 2 43
1.584073	46	10.767851	47.639448	110.767851	64 31 46
1.599994	47	11.249817	48.749582	111.249817	64 0 39
1.616074	48	11.742877	49.864522	111.742877	63 29 49
1.632315	49	12.247092	50.984407	112.247092	63 59 7
1.648721	50	12.762587	52.109512	112.762587	62 28 34

Table II. *continued.*—The Ordinary Catenary.

$a = 100$.

N.	y.	x.	z.	T.	Angle.		
					°	′	″
1.665290	51	13.289300	53.239600	113.289300	61	58	9
1.682027	52	13.827388	54.375311	113.827388	61	27	53
1.698932	53	14.376853	55.516346	114.376853	60	57	45
1.716006	54	14.937727	56.662872	114.937727	60	27	46
1.733252	55	15.510107	57.815092	115.510107	59	57	56
1.750672	56	16.094061	58.973138	116.094061	59	28	14
1.768266	57	16.689588	60.137011	116.689588	58	58	42
1.786037	58	17.296790	61.306900	117.296790	58	29	19
1.803988	59	17.915770	62.483020	117.915770	58	0	5
1.822118	60	18.546493	63.665306	118.546493	57	31	1
1.840431	61	19.189099	64.854000	119.189099	57	2	5
1.858927	62	19.843586	66.049113	119.843586	56	33	20
1.877610	63	20.510098	67.250901	120.510098	56	4	43
1.896480	64	21.188633	68.459366	121.188633	55	36	16
1.915540	65	21.879300	69.674600	121.879300	55	7	59
1.934792	66	22.582171	70.897028	122.582171	54	39	52
1.954237	67	23.297283	72.126416	123.297283	54	11	54
1.973877	68	24.024709	73.362990	124.024709	53	44	6
1.993715	69	24.764560	74.606930	124.764560	53	16	28
2.013752	70	25.516873	75.858326	125.516873	52	48	59
2.033990	71	26.281725	77.117274	126.281725	52	21	41
2.054433	72	27.059265	78.384034	127.059265	51	54	33
2.075080	73	27.849426	79.658573	127.849426	51	27	34
2.095935	74	28.652451	80.941048	128.652451	51	0	46
2.117000	75	29.468327	82.231672	129.468327	50	34	8
2.138276	76	30.297123	83.530476	130.297123	50	7	40
2.159766	77	31.138956	84.837643	131.138956	49	41	22
2.181472	78	31.993903	86.153296	131.993903	49	15	14
2.203396	79	32.862044	87.477555	132.862044	48	49	16
2.225540	80	33.743457	88.810542	133.743457	48	23	29
2.247907	81	34.638263	90.152436	134.638263	47	57	52
2.270500	82	35.546581	91.503418	135.546581	47	32	25
2.293318	83	36.468371	92.863428	136.468371	47	7	8
2.316366	84	37.403837	94.232762	137.403837	46	42	2
2.339646	85	38.353056	95.611543	138.353056	46	17	6
2.363160	86	39.316110	96.999880	139.316110	45	52	20
2.386910	87	40.293084	98.397915	140.293084	45	27	45
2.410900	88	41.284143	99.805856	141.284143	45	3	20
2.435129	89	42.289243	101.223656	142.289243	44	39	5
2.459602	90	43.308592	102.651607	143.308592	44	15	1
2.484322	91	44.342313	104.089886	144.342313	43	51	7
2.509290	92	45.390455	105.538544	145.390455	43	27	23
2.533983	93	46.430931	106.967368	146.430931	43	4	18
2.559981	94	47.530444	108.467655	147.530444	42	40	26
2.585709	95	48.622506	109.948393	148.622506	42	17	13
2.611696	96	49.729447	111.440152	149.729447	41	54	10
2.637944	97	50.851184	112.943315	150.851184	41	31	18
2.664455	98	51.988313	114.457186	151.988313	41	8	36
2.691234	99	53.140537	115.982862	153.140537	40	46	4
2.718281	100	54.308027	117.520072	154.308027	40	23	42

Table III. The Catenary of equal strength.

$y = 100$.

a.	*x.*	*z.*	ζ.	T.	Angle.		
					°	′	″
1000	5.008288	100.166600	100.334300	1005.020800	84	16	13
980	5.110881	100.173640	100.348276	985.124220	84	9	12
960	5.217781	100.181250	100.363200	965.232000	84	1	54
940	5.329126	100.188850	100.378652	945.344276	83	54	16
920	5.445471	100.197071	100.395276	925.461672	83	46	19
900	5.566977	100.202654	100.413000	905.584230	83	38	1
880	5.694003	100.215533	100.432288	885.712432	83	29	20
860	5.827073	100.225792	100.452730	865.846882	83	20	15
840	5.966506	100.237329	100.475340	845.987772	83	10	44
820	6.112609	100.247806	100.497724	826.135404	83	0	45
800	6.266274	100.261054	100.523680	806.290880	82	50	16
780	6.427811	100.274596	100.551048	786.454344	82	39	15
760	6.598152	100.289657	100.580680	766.626896	82	27	40
740	6.777369	100.305695	100.613064	746.808518	82	15	25
720	6.966790	100.322732	100.647648	727.000675	82	2	32
700	7.167238	100.342923	100.685480	707.204050	81	48	53
680	7.379542	100.362168	100.726972	687.419752	81	34	26
660	7.604848	100.384645	100.772166	667.647826	81	19	7
640	7.844443	100.409125	100.821568	647.892736	81	2	51
620	8.099715	100.436355	100.876232	628.152876	80	45	31
600	8.370382	100.465969	100.936080	608.430840	80	27	2
580	8.663690	100.498855	101.002534	588.728710	80	7	17
560	8.976381	100.535447	101.076360	569.048704	79	46	7
540	9.312582	100.576282	101.158740	549.393354	79	23	23
520	9.675126	100.621836	101.250968	529.765704	78	58	53
500	10.067350	100.679481	101.362400	510.169400	78	32	27
480	10.552010	100.780247	101.472192	490.668864	78	3	48
460	10.956213	100.796941	101.605490	471.087748	77	32	39
440	11.462781	100.872044	101.757920	451.613404	76	58	41
420	12.018908	100.958305	101.933328	432.192558	76	21	29
400	12.630692	101.056700	102.136560	412.832200	75	40	33
380	13.312576	101.174410	102.373976	393.548520	74	55	19
360	14.071210	101.311236	102.653784	374.349852	74	5	4
340	14.922900	101.473699	102.986884	355.255222	73	8	53
320	15.886128	101.668413	103.387488	336.287040	72	5	42
300	16.984763	101.904940	103.875990	317.474760	70	54	5
280	18.250135	102.196102	104.480264	298.858028	69	32	14
260	19.729226	102.564124	105.241136	280.497074	67	57	47
240	21.465587	103.025715	106.219200	262.454784	66	7	36
220	23.555838	103.632647	107.507994	244.863168	63	57	23
200	26.116574	104.447443	109.260480	227.898480	61	21	7
180	29.336487	105.580330	111.739482	211.862484	58	10	8
160	33.525185	107.228464	115.437376	197.296208	54	11	24
140	39.241137	109.779803	121.380952	185.292618	49	4	28
120	47.626016	114.104417	132.093348	178.461912	42	15	12
100	61.562643	122.619114	155.740770	185.081570	32	42	15
95	66.748734	126.148321	166.629316	191.808059	29	41	19
90	73.141390	130.727676	181.797084	202.855068	26	20	16
85	81.313401	136.905055	204.267512	221.246959	22	35	35
80	92.332784	145.717467	240.765568	253.708616	18	22	48
75	108.536763	159.466590	309.878850	318.825817	13	36	20
70	136.763450	184.926359	488.855143	493.841432	8	8	56

Table IV.—The Catenary of equal strength.

$a = 100$.

y.	*x.*	*z.*	ζ.	T.	Angle.		
					°	′	″
1	.004999	.999990	1.00001	100.00500	89	25	37
2	.020003	2.000088	2.00022	100.020006	88	51	14
3	.045005	3.000431	3.00088	100.045016	88	16	52
4	.080021	4.001021	4.00208	100.080054	87	42	29
5	.125046	5.002067	5.00415	100.125125	87	8	6
6	.180107	6.003541	6.00714	100.180270	86	33	44
7	.245198	7.005697	7.01143	100.245499	85	59	21
8	.323389	8.008498	8.01706	100.320852	85	24	58
9	.405548	9.012161	9.02436	100.406373	84	50	46
10	.500828	10.016660	10.03343	100.502080	84	16	13
11	.606218	11.022229	11.04456	100.608062	83	41	50
12	.721234	12.028425	12.05789	100.723845	83	7	28
13	.847386	13.036754	13.07372	100.850992	82	33	5
14	.983205	14.045921	14.09215	100.988063	81	58	42
15	1.129248	15.056560	15.11351	101.135644	81	24	20
16	1.285490	16.068670	16.13791	101.293792	80	49	57
17	1.452011	17.082468	17.16567	101.462608	80	15	34
18	1.628815	18.097959	18.19691	101.642158	79	41	12
19	1.815961	19.115360	19.23197	101.832558	79	6	49
20	2.013470	20.134658	20.27097	102.033880	78	32	23
21	2.221395	21.156371	21.31424	102.246255	77	58	4
22	2.439770	22.179619	22.36191	102.469780	77	23	41
23	2.668651	23.205504	23.41433	102.704585	76	49	19
24	2.908061	24.233742	24.47164	102.950768	76	14	56
25	3.158106	25.264601	25.53424	103.208504	75	40	33
26	3.418774	26.297360	26.60212	103.477887	75	6	11
27	3.690164	27.334154	27.67581	103.759100	74	31	48
28	3.972311	28.373174	28.75540	104.052264	73	57	25
29	4.265294	29.415243	29.84128	104.357567	73	23	3
30	4.569158	30.460378	30.93360	104.675156	72	48	40
31	4.883983	31.508739	32.03269	105.005213	72	14	17
32	5.209839	32.560521	33.13891	105.347935	71	39	55
33	5.546782	33.615738	34.25243	105.703501	71	5	32
34	5.894915	34.674639	35.37366	106.072131	70	31	9
35	6.254281	35.737235	36.50280	106.454005	69	56	47
36	6.624997	36.803792	37.64030	106.849383	69	22	24
37	7.007106	37.874291	38.78626	107.258446	68	48	2
38	7.400749	38.948988	39.94126	107.681495	68	13	39
39	7.805967	40.027947	41.10545	108.118722	67	39	16
40	8.222888	41.111407	42.27931	108.570433	67	4	54
41	8.651589	42.199404	43.46308	109.036870	66	30	31
42	9.092196	43.292198	44.65724	109.518354	65	56	8
43	9.544771	44.389841	45.86509	110.015128	65	21	46
44	10.009478	45.492556	47.07804	110.527566	64	47	23
45	10.486371	46.600436	48.30547	111.042096	64	13	0
46	10.975622	47.713735	49.54487	111.600602	63	38	38
47	11.477312	48.832499	50.79655	112.161892	63	4	15
48	11.991595	49.957023	52.06108	112.740211	62	29	52
49	12.518572	51.088569	53.34078	113.335897	61	55	32
50	13.058418	52.223810	54.63024	113.949396	61	21	7

Table IV. *continued.*—The Catenary of equal strength.

$a = 100.$

y.	*x.*	*z.*	ζ.	T.	Angle.		
					°	′	″
51	13.611226	53.366417	55.93584	114.581052	60	46	44
52	14.177189	54.515494	57.25618	115.231377	60	12	22
53	14.756401	55.676950	58.59167	115.900748	59	37	59
54	15.349077	56.833577	59.94296	116.589191	59	3	36
55	15.955315	58.002974	61.31049	117.298661	58	29	14
56	66.575346	59.179619	62.69495	118.028208	57	54	51
57	17.209276	60.363609	64.09682	118.778802	57	20	29
58	17.857313	61.555215	65.51678	119.551032	56	46	6
59	18.519676	62.754711	66.95554	120.345521	56	11	43
60	19.196491	63.962210	68.41362	121.162801	55	37	21
61	19.888020	65.178046	69.89186	122.003580	55	2	58
62	20.594400	66.402358	71.39084	122.868440	54	28	35
63	21.315910	67.635500	72.91145	123.758155	53	54	13
64	22.052701	68.877606	74.45432	124.673361	53	19	50
65	22.805074	70.129059	76.02042	125.614906	52	45	27
66	23.573186	71.389994	77.61043	126.583487	52	11	5
67	24.357371	72.660825	79.22540	127.580036	51	36	42
68	25.157787	73.941697	80.86608	128.605306	51	2	19
69	25.974778	75.233031	82.53360	129.660301	50	27	57
70	26.808551	76.535188	84.22878	130.745895	49	53	34
71	27.659459	77.848058	85.95285	131.863168	49	19	11
72	28.527710	79.172384	87.70674	133.013056	48	44	49
73	29.413697	80.508436	89.49175	134.196771	48	10	26
74	30.317647	81.856432	91.30890	135.415343	47	36	4
75	31.239989	83.216866	93.15964	136.670112	47	1	41
76	32.180961	84.589966	95.04510	137.962209	46	27	18
77	33.140961	85.975963	96.96618	139.293095	45	52	56
78	34.120421	87.375961	98.92611	140.664048	45	18	33
79	35.119618	88.789594	100.92453	142.076604	44	44	10
80	36.139051	90.214639	102.96381	143.532386	44	9	48
81	37.179043	91.660396	105.04542	145.032900	43	35	25
82	38.240111	93.118455	107.17133	146.579992	43	1	2
83	39.322622	94.592159	109.34320	148.175357	42	26	40
84	40.427139	96.082135	111.56319	149.821051	41	52	17
85	41.554052	97.588753	113.82816	151.518952	41	17	54
86	42.703981	99.112699	116.15555	153.271369	40	43	32
87	43.877350	100.654374	118.53239	155.080397	40	9	9
88	45.074822	102.214506	120.96637	156.948608	39	34	46
89	46.296874	103.793554	123.45986	158.878369	39	0	24
90	47.544231	105.392291	126.01578	160.872559	38	26	1
91	48.817411	107.011233	128.63685	162.933851	37	51	39
92	50.117199	108.651210	131.32634	165.065469	37	17	16
93	51.444173	110.312786	134.08729	167.270444	36	42	53
94	52.799201	111.996881	136.92343	169.552431	36	8	31
95	54.182891	113.704104	139.83816	171.914846	35	34	8
96	55.596244	115.435462	142.83573	174.361831	34	59	45
97	57.039914	117.191641	145.92002	176.897299	34	25	23
98	58.514946	118.973717	149.09580	179.525931	33	51	0
99	60.032087	120.782488	152.36759	182.252247	33	16	37
100	61.562647	122.619117	155.74077	185.081573	32	42	15

XVI. *On magnetic influence in the solar rays.* *By* SAMUEL HUNTER CHRISTIE, *Esq. M. A. F. R. S. of Trinity College, Cambridge; Fellow of the Cambridge Philosophical Society: of the Royal Military Academy. Communicated November* 15, 1825.

Read January 19, 1826.

THE object of the present communication is to show, by a series of experiments, that the solar rays possess sensible magnetic properties, which are observable in the vibrations of a magnetised needle exposed to those rays, independently of the effects produced by the heat which they impart. I propose likewise to point out the changes which take place in the intensity of a magnetised needle from changes of temperature, as deducible from the times of its vibration. I have before stated in my paper *on the effects of temperature on the intensity of magnetic forces*, that, in deducing the terrestrial magnetic intensity by means of the vibrations of a needle, a correction ought to be introduced where the observations have been made at different temperatures. I had not at the time made any experiments by which I could ascertain how far changes in the temperature of a needle would be sensible in the time of its vibration; and the first observations which I made with this view being with a light needle, did not give very decided results: they however led me first to notice the very singular fact, that a magnetic needle comes to rest more quickly when vibrated exposed to the rays of the sun, than when vibrated in the shade.

In order to ascertain the effect which changes of temperature have on the times of vibration of a needle, it is necessary to know the temperature of the needle itself during the observations, and I saw no better means of ascertaining this, even approximately, than to vibrate it in the shade and then exposed to the rays of the sun, and to consider the temperature of the needle to be that indicated by a thermometer near to it. On my first doing this, I found, that although I could easily mark the 50th vibration when the needle was shaded, I could not distinguish beyond the 40th when it was exposed. I at the same time found that the time of vibration was slightly diminished at the higher temperature, instead of being increased, as I had reason to expect. As however the needle was not vibrated in the same spot in the two cases, the diminution in the time of vibration and of the arc when it was exposed, might be independent of the change of temperature and of any influence in the solar rays. To avoid any uncertainty arising from difference of disturbing causes in two situations, I placed the compass out of doors, with a screen composed intirely of wood, supported at the height of four feet above it, and by removing which the rays of the sun struck directly on the needle. A thermometer having the bulb near to the compass-box indicated nearly the temperature of the needle. When the shutter was up, so that the needle vibrated in the shade, I could very distinctly note the 100th vibration; but when it was removed and the needle vibrated exposed to the sun's rays, I could not so distinctly mark the 75th. I made use of a needle six inches long, weighing 42.75 grains, and contained in a brass compass-box with a glass cover: the needle was suspended by a fine hair, and commenced vibrating 30° from zero.

Needle exposed to the sun; or being in shade.	Thermometer.	Time of performing 50 vibrations.	Extent of arc of vibration from zero at 50th vibration.
Shade . .	60.9	118.8 seconds.	Not observed.
Sun . .	91.5	118.0	Not observed.
Shade . .	76.0	118.8	5° 00′
Sun . .	75.3	118.0	2 30
Sun . .	90.4	118.4	2 45
Sun . .	91.4	118.0	2 30
Sun . .	89.4	118.4	2 30
Shade . .	81.6	118.7	4 45

From these observations it is evident, that although the needle vibrated rather more rapidly, yet it came more quickly to a state of rest when exposed to the rays of the sun, than when in the shade. The latter effect appears to have been produced independently of the heat of the rays; since when the thermometer had risen only to 75.3, the terminal arc had decreased from 5° to 2° 30′. These observations were made between noon and 1^{h} 30^{m} on the 4th of June 1824, and I had no opportunity of repeating them during that summer.

As the apparent increase of intensity at an increased temperature, indicated by a small diminution in the time of vibration, excited my surprise as much as the more rapid decrease of the arcs of vibration when the needle was exposed to the rays of the sun, I resolved to make use of a balance of torsion, in order to deduce the changes of intensity from the deviation caused by torsion, as well as from the times of vibration; and that there might be less ambiguity in the results, I employed a much more powerful needle, its weight being 197 grains, and length 6 inches as before. This needle was suspended by a wire (No. 22) 22.9 inches long, and free from torsion when the needle pointed to zero. The observations contained in the following table were made between 1 and 2 o'clock after noon on the 30th of May, 1825; as before, the vibrations commenced 30° from zero.

Needle exposed to the sun; or being in shade.	Thermometer.	Index on the micrometer of torsion.	Deviation of the needle.	Time of performing 50 vibrations.	Extent of arc of vibration from zero at 50th vibration.
Shade.	60	0°	- - -	158.35 seconds.	5° 45′
	60	0	- - -	158.25	5 40
	61	360	19 00		
Sun, but not strong.	73	360	19 10		
	82	360	19 16		
	80	0	- - -	158.7	3 10
	85	0	- - -	158.7	3 15
	87	0	- - -	159.3	3 05

Here, as before, the terminal arc was in all cases diminished when the needle vibrated exposed to the solar rays, but the increase of temperature caused a diminution of intensity, whether the intensity be estimated by the time of vibration or by the deviation caused by torsion. Estimating it by the latter, we shall have, since the directive force varies directly as the torsion and inversely as the sine of the deviation,

Directive force at temperature 61° $= \frac{341}{\text{sin. } 19^\circ} = 1047.186$;

Directive force at temperature 82° $= \frac{340.73}{\text{sin. } 19^\circ\, 16'} = 1032.624$.

So that with an increase of temperature of 21° there appears to have been a decrease of intensity of 14.552. This would give the decrement of intensity .00067 for an increment of 1° in temperature between the temperatures 61° and 82°, the intensity at 61° being considered 1. From the experiments with powerful magnets, detailed in my former paper (Phil. Trans. 1825. p. 1.) it appears that, considering the intensity at 62° as 1, the decrement of intensity corresponding to an increment of temperature of 1°, between 62.05 and 77.65, would be .000586, from which the foregoing result does not differ greatly.

If we estimate the change of intensity by the difference in the times of vibration, calling the intensity at the temperature 60°, 1; we shall have,

$$\text{Intensity at temperature } 84^\circ = \frac{(158.3)^2}{(158.9)^2} = .992462.$$

We have therefore here a decrease of intensity .007538, in consequence of an increase of 24° in temperature; or a decrement .000314 corresponding to an increment 1° in temperature, between the temperatures 60° and 84°. This is considerably less than was deduced from the deviation due to torsion, which arises in some measure from the acceleration from torsion being very nearly constant at different temperatures, and therefore tending to diminish the difference in the times of vibration.

The following observations were made between $11^h\ 30^m$ and $12^h\ 40^m$ of the 10th of June, 1825, under very favourable circumstances, the sun shining clear and strong the whole time. The heavy needle employed in the last observations was again made use of; it now rested on the pivot. The point where the vibrations commenced for each 40 vibrations was 90° from zero.

Needle exposed to the sun; or being in shade.	Thermometer.	Time of performing 40 vibrations.	Extent of arc of vibration from zero at 40th vibration.	Times of commencing and of concluding the observations.
Shade.	72.5	135.0 seconds.	15° 30′	$11^h 30^m$
	75.0	135.0	14 30	
	75.0	135.0	13 30	
	76.0	134.8	12 00	11 52
Mean .	74.6	134.95	13 52	
Sun.	90.0	135.3	8 00	11 55
	105.0	135.7	9 00	
	109.0	136.2	8 30	
	112.0	136.2	8 20	12 12
Mean .	104.0	135.85	8 27	
Shade.	87.0	135.4	12 30	12 20
	84.0	135.5	14 00	
	82.0	135.3	13 00	
	81.0	135.1	12 00	12 40
Mean .	83.5	135.32	12 57	

Deducing the changes of intensity from the times of vibration, the observations at 74°.6 and 104° give .00045, and those at 83°.5 and 104° give .00038 as the decrement in intensity corresponding to an increment in temperature of 1°, one of the observations being in the shade, and the other in the sun in each case; but from the observations at 74°.6 and 83°.5, where both were made in the shade, we have .00061: and again, from the observations at 90° and 110°.5 (the mean of 109° and 112°, the times of vibration at these temperatures being the same,) where both were made in the sun, we obtain .00064 as the decrement, the intensity at the lower temperature being in each case considered as 1. The small disagreement in the first two results may arise from errors in determining the temperature of the needle at the time of vibration, which, from the rapid changes that took place, could not be ascertained with precision; and the much greater difference between either of these and the other two may possibly be attributable to the same source, though I shall have occasion at the conclusion of this paper to point out another, and, I conceive, the true cause of this difference. Had my object been to give a correction for difference of temperature, to be applied generally to the intensities deduced from the times of vibration, a closer agreement in the results would have been desirable; but although such is not my object, I wish, from these experiments, to point out the necessity of such correction, when we would deduce the magnetic intensity from the times of vibration of a needle in different parts of the earth, where the temperatures during the observations are necessarily different.

The observations which I have detailed are, I think, quite conclusive as establishing the fact, that the rays of the sun had a tendency to check the vibrations of the needle, particularly those in the last table; since here, on the needle being exposed to the sun, the terminal arc was reduced from nearly 14° to 8½°, and again on excluding its rays that arc was increased to nearly 13°: the only question is whether this would take place under all circumstances; that is, whether the influence be exerted in all cases directly on a magnetised needle and independently of changes in its temperature and intensity, or whether it was an effect produced on the metal of the box by a change in its temperature. That the effect is independent of changes in the temperature and intensity of the needle will appear from comparing the first observation in the sun with the first of the second set in the shade; the temperatures and times of vibration are very nearly the same in the two cases, and yet in the sun the terminal arc is only 8°, whereas it is 12° 30′ in the shade. That the diminution of the arc does not arise from an increase of temperature in the box appears from this, that although the thermometer indicated 90° at the first observation in the sun, and 87° at the first in the shade, it is probable that the temperature of the brass compass-box was higher in the latter case than in the former, since in both this would change its temperature more slowly than the mercury in the bulb of the thermometer.

To ascertain beyond doubt whether these conclusions were correct, I placed the compass in a room where it was not exposed to the rays of the sun, and when the needle had acquired the temperature of the room, I vibrated it, and noted the time of 40 vibrations and the arc at the 40th; I then heated the compass-box over a fire until its heat was barely supportable to the hand, replaced and vibrated the needle. Instead of finding that the increased temperature of the box had diminished the terminal arc, I found that, at the end of the first 40 vibrations, it was considerably greater than when the box had been at a lower temperature, and that, as the box had cooled, the terminal arcs in the successive 40 vibrations decreased, the vibrations always commencing from 90°; and this effect was invariably produced in repeated trials. A thermometer was placed at a small distance from the compass, that any changes in the temperature of the air might be indicated: this remained nearly at the same point throughout the observations.

		Thermometer.	Time of performing 40 vibrations.	Extent of arc of vibration from zero at 40th vibration.
Observations previous to heating the compass-box.		63.0	139.65 seconds.	15° 00′
			139.2	13 00
			139.25	13 00
		64.0	139.4	13 00
Observations after the compass-box had been heated.	1st time	64.5	141.3	21 00
			139.6	12 45
			139.3	12 30
		64.7	139.5	12 30
	2d time	64.0	141.6	20 00
			140.0	15 00
			140.2	15 00
		63.5	140.0	15 30
	3d time	63.4	141.4	20 00
			140.2	14 30
			139.6	12 45
		63.0	139.7	12 00

These observations show clearly that an increased temperature on the compass-box tended to increase considerably the terminal arc, and that if the increased temperature, and consequent diminished intensity of the needle, had any tendency to diminish that arc, it was much more than counterbalanced by the effect produced on the compass-box. That such an effect should be produced on the brass is singular, and can, I think, only be accounted for on the supposition, that the capacity in brass for the developement of magnetism by induction is diminished by an increase of temperature, the vibrations of the needle being in all cases checked by the magnetism developed in the brass by the needle itself.*

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* Since making the foregoing observations I have found that a similar effect, arising, I conceive, from the same cause, is produced on a copper disc vibrating under the influence of a magnet: the number of vibrations required to reduce the

I now proposed observing the effect on the terminal arc of a change of temperature, and consequently a change of intensity in the needle, other circumstances remaining precisely the same. For this purpose, I plunged the ends of the needle nearly to its centre into boiling water, and vibrated it as soon as possible afterwards. On my first doing this the pivot on which the needle vibrated was slightly injured, so that being obliged to re-magnetise the needle, in order to obtain results previous to a diminution of intensity to be compared with those after the needle had been immersed, these results could not be compared with those which I had before obtained. I mention this, to guard against any conclusions being drawn from a comparison of the times of vibration or of the terminal arcs in the following observations with those of the preceding. In consequence of the increased friction on the pivot, the arc of vibration was reduced to 5½° on each side of zero at the 30th vibration, I therefore only observed that number: the vibrations commenced, as before, 90° from zero.

arc of vibration to a certain extent was, by heating the disc, increased from being less than 23 to 26½, taking a mean of several observations. The method of vibrating metallic discs between the poles of a magnet, which I here made use of, was, I believe, first employed by Mr. WILLIAM STURGEON, of Woolwich, for the purpose of exhibiting the magnetic properties of different metals, which it does in a very striking manner.

	Thermometer.	Time of performing 30 vibrations.	Extent of arc of vibration from zero at 30th vibration.
The needle having been re-magnetised previous to immersion in boiling water.	63°	99.9 seconds.	5° 15′
		99.8	5 30
		99.7	5 30
		99.7	5 30
	Mean	99.775	5 26
The needle having been immersed in boiling water.	63	104.2	6 00
		104.15	6 15
		104.25	6 15
		104.4	6 15
	Mean	104.25	6 11
The needle having been re-magnetised after immersion in boiling water.	63	100.2	6 15
		100.4	6 30
		100.3	6 30
		100.4	6 45
	Mean	100.325	6 30

The increase here in the terminal arcs being gradual, and taking place under opposite circumstances, appears to have arisen from the friction on the pivot being gradually diminished. Taking a mean of the arcs before immersion and after it, when the needle had been re-magnetised, it is rather less than the mean arc when the intensity of the needle was reduced by an increased temperature arising from the immersion; so that an increase of temperature which diminished the intensity of the needle from 1 to 0.92, produced no sensible effect on the terminal arc. The decrease of intensity is here much greater than any that could arise from an increase of temperature derived from the direct rays of the sun; it is therefore manifest from these last experiments, independent of any thing which I have before stated, that the heat of the sun, by diminishing the intensity, could not be the cause of the diminution of the terminal arc. That this diminution could not arise from the increased temperature of the compass-box, the previous experiments have, I trust, satisfactorily proved: I think I may therefore safely conclude, that during the vibration of a magnetised needle exposed to the solar rays, these rays have an influence on the needle independent of the effect produced by heat, which tends to bring the needle to a state of rest.

In the first observations which I have described it appears that, although we might expect that, when the needle vibrated in the sun, the increased temperature would diminish the intensity, yet a slight acceleration took place in the time of vibration. Whether this is to be attributed to the same cause or not, it is perfectly analogous to what I have observed on vibrating a metallic disc under the influence of a magnet: the arc of vibration was much more rapidly diminished than when the disc vibrated uninfluenced by the magnet, and the times of vibration were also diminished. A copper disc 7 inches in diameter having its axis of suspension perpendicular to its plane, and at a small distance from its centre, made 286 vibrations before the arc of vibration was reduced to a certain extent; but on vibrating it between the poles of a horse-shoe magnet it made only 23 vibrations before the arc was reduced to the same extent. The times in which the disc performed 24 vibrations at different trials were:

	Freely.	Between the poles of the magnet.
24 vibrations in .	35.15 seconds . .	34.6 seconds.
24	35.15	34.7
24	35.1	34.6
24	35.18	34.6
Time of 1 vibration . .	1.4635	1.4427

so that another effect of the magnet was to diminish the time of vibration of the disc. If the acceleration in the time of vibration of the needle when exposed to the sun's rays arise from a similar cause, we ought also to find that the decrement of intensity in the needle corresponding to an increase of temperature, as deduced from the times of vibration in the sun and in the shade, is less than the decrement deduced from the times of vibration at different temperatures when both observations were made in the shade, or both in the sun, and likewise less than that deduced from the deviation due to torsion; and this, from the observations, appears to have been decidedly the case. Probably, however, the accelerations in the times of vibration are only consequences of the diminution of the arcs, both in the case of the copper disc and of the needle vibrating in the sun: still this acceleration will account for these differences in the decrements of intensity.

Although we should not at present be able to point out the manner in which the rays of the sun tend to bring the needle to rest, the discovery of a fact proving magnetic influence in the solar rays must, I think, ultimately lead to important conclusions respecting the phænomena of terrestrial magnetism. On this account it is desirable that the mode of observing should be varied, and that observations should be made in climates more favourable for the purpose than our own. I regret that, in consequence of my absence from home, I could not repeat the experiments during the extremely hot and clear weather of the past summer, as it is probable that the effects might have been even more decided than those which I have detailed. Possibly the effect might be greatly increased by placing the needle so nearly in the focus of a large lens or reflector, that the rays should be concentrated on the space in which the needle vibrated. In this case, however, the intensity of the needle might be so much diminished by the high temperature, as to render doubtful the cause of the effect observed.

The repeated failures of MORICHINI's experiment of magnetising a needle by the violet ray, even under the most favourable circumstances, and in the ablest hands, have led many to doubt whether the effects, which were in some cases observed, were to be attributed to the influence of the ray; but as the experiments which I have detailed indicate magnetic influence in the compound solar rays, and are besides easily repeated, they will, I think, tend considerably to remove these doubts.

Royal Military Academy,
November 15, 1825.

Additional observations.

Since the foregoing paper was read, having made some further observations which show more decidedly than the preceding that the effect which I have described is due to magnetic influence in the solar rays, I beg here to add them. I was satisfied that the decrease in the terminal arc, when the needle vibrated in the sun, could not be due to the increased temperature of the metallic compass-box in which the needle vibrated, since in the experiment which I have described, and which I have carefully repeated, the effect produced by heating the box was to increase considerably that arc; yet I considered that it must remove all doubt on the subject, if the box in which the needle vibrated were of wood, or other non-metallic substance. I resolved therefore at the earliest opportunity to repeat my experiments, and to make additional ones with an apparatus, in the construction of which I had scrupulously excluded metal, the suspension of the needle alone excepted. The compass-box is of mahogany, the sides being of thick pasteboard: the graduations are on paper. Through a perforation in the middle of the glass cover a glass tube is fixed, and down this the suspending wire, which is fixed above, passes into the compass-box. The needle can be released 90° from zero by means of a wooden or glass pin, turning in the side of the box. It would have been very desirable to have repeated the experiments in a vacuum, as the effect which I had observed to be produced by vibrating the needle exposed to the sun, might possibly be attributed to changes taking place in the medium in which the needle moved; but as there would be extreme difficulty in doing this without having metal in the vicinity of the needle, I proposed to vibrate needles of copper and of glass, by the force of torsion, both in the shade and exposed to the sun, to observe the terminal arcs under those circumstances, and to compare the effects with those obtained with a magnetised needle. The needles employed are all of the same form; their length 6 inches, breadth in the middle 1.5 inches, their sides being nearly circular arcs. The magnetised needle was suspended by a very fine hard brass wire, No. 35, between $\frac{1}{300}$ and $\frac{1}{400}$ inch in diameter, and 10 inches in length; the glass needle was suspended by the same length of No. 18; and the copper needle by the same length of this wire doubled. In vibrating these needles they were released 90° from zero; and in order to be certain that the extent of the first vibration was the same when the needle vibrated in the sun as when it vibrated in the shade, I noted also the extent of the arc of vibration on the other side of zero. The observations were made in the open air at a distance from my house: the compass-box was placed on a wooden tripod about 2 feet from the ground, and 5 feet from the north side of a garden wall, on which a wooden screen, moveable at pleasure, rested $3\frac{1}{2}$ feet above the box. The following are the results which I obtained.

Nature and Weight of the needle vibrated.	No. of vibrations performed.	Needle vibrated in the sun.				Needle vibrated in the shade.				Terminal excess.
		Extent of first vibration.	Time.	Terminal arc.	Therm.	Extent of first vibration.	Time.	Terminal arc.	Therm.	
		° °	m. sec.	°	°	° °	m. sec.	°	°	°
Magnetised steel,	100	90 + 90	5 55.4	20	100	90 + 89¾	5 58.6	33	47	13
	100	90 + 89¾	5 55.2	19¾	104	90 + 90	5 58.8	33¾	48	14
	100	90 + 89¾	5 55.1	19½	106	90 + 89¾	5 58.8	33¾	46½	14¼
Weight = 225½ grains.	Means	90 + 89⅚	5 55.23	19¾	103.3	90 + 89⅚	5 58.7	33½	50.5	13¾
		1st April, from 9h 47m to 10h 15m.				1st April, from 10h 19m to 10h 45m.				
Glass,	100	90 + 88	6 27.2	17	96	90 + 88	6 27.0	23	49	6
	100	90 + 88	6 27.1	18	99	90 + 88	6 27.2	23	47	5
	100	90 + 88	6 27.2	17¾	100	90 + 87¾	6 27.1	22	47	4¼
Weight = 224½ grains.	Means	90 + 88	6 27.17	17 7/12	98.3	90 + 88	6 27.1	22⅔	47.7	5 1/12
		1st April, from 10h 58m to 11h 27m.				1st April, from 11h 53m to 0h 20m:				
Copper,	100	90 + 93	7 40.2	24	93	90 + 93½	7 39.6	30¼	51	6¼
	100	90 + 94	7 40.2	24	98	90 + 94	7 39.4	31	49½	7
	100	90 + 93½	7 40.0	24	103	90 + 93¾	7 39.5	31	49½	7
Weight = 543 grains.	Means	90 + 93½	7 40.13	24	98	90 + 93¾	7 39.5	30¾	50	6¾
		1st April, from 1h 20m to 1h 53m.				1st April, from 2h 5m to 2h 35m.				

In whatever way we may attempt to account for the terminal arc of vibration, when a needle is screened from the sun, exceeding that when it vibrates exposed to the rays, (which excess I have called the terminal excess) the result here obtained with the glass needle is certainly extraordinary: as compared with that obtained with the magnetised needle, it shows decidedly that the solar rays exert a peculiar influence on a magnetised needle during its vibration: considered separately, either that glass is magnetic, which is scarcely probable, since its magnetism is not detected by rotating a magnet under it, or that the rays produce an effect distinct and independent of their magnetical effect.

These observations were made consecutively without even moving the compass-box; but it is scarcely possible, especially at the present season of the year, to make such a series under precisely the same circumstances of temperature of the air and of solar heat, by which alone we can in this case judge of the intensity of the action of the sun. The change which took place during the observations was probably not quite so great as I have registered, since in the first observation with the magnetised needle the glass had been closed over the thermometer for some time before I registered its height; and in the first observations with the other needles it had been open and in the shade, for the preceding observations, until very nearly that time; so that 100,99 and 98 would probably indicate more correctly the comparative elevations of the thermometer in the first observations with the different needles than 100,96 and 93, which I have registered. But even taking the latter, the small difference 5° would produce little difference in the results, and is much more than counterbalanced by the time during which the glass and copper needles vibrated being greater than that for the magnetised needle. If the effects be reduced proportionally to the times, the terminal excess in the same time would be for the magnetised needle 13°.75, for the copper needle 5°.24, and for the glass needle 4°.71.

The diminution in the time of vibration of the magnetised needle when exposed to the sun, may at first sight appear singular, since there can be no doubt that, the temperature being higher, the intensity of the needle was less than when it was screened. This diminution in the time of vibration I have no doubt arose principally from the more rapid diminution of the arcs of vibration in the former case than in the latter, and likewise in some measure from the needle not recovering the same intensity of magnetism after being exposed to the high temperature of the sun's rays which it previously possessed. That these causes operated to a great extent is clearly shown by the following observations which I afterwards made. The same needle, but having its weight increased to 252 grains, was first vibrated in the shade from 90° as before, and three different observations gave me $5^m\ 58^s.8$, $5^m\ 58^s.6$, $5^m\ 58^s.6$, as the time of performing 100 vibrations, or $3^s.587$ as the mean time of 1 vibration, the terminal arc being 33 and thermometer 64.5. I then vibrated it, still in the shade, from 20°, and found that 50 vibrations were performed in $2^m\ 48^s.8$, giving $3^s.376$ as the mean time of vibration, the terminal arc being 13° and thermometer 65°. Similar observations being immediately afterwards made with the needle exposed to the sun, gave $5^m\ 59^s.4$, $5^m\ 59^s.4$, and $6^m\ 00^s$, as the time of performing 100 vibrations, or

3'.596 as the mean time of one vibration, when the needle vibrated from 90°, the terminal arc being 19⅓° and thermometer 102.3; and vibrated from 20°, the time of performing 50 vibrations was 2 52.2, or the mean time of one vibration 3.445, the terminal arc being 9° and thermometer 110. The intensities in the shade and in the sun, estimated by the times of vibration in the large arcs would be 1 and .995; but estimated by the times of vibration in the small arcs 1 and .960. It appears from these observations, that in deducing the magnetic intensity in different parts of the earth from the vibrations of a needle, it is very essential to attend to the circumstance of the needle having been exposed or not to the rays of the sun during its vibration, as well as to the temperature in which the vibrations have been made.

The very small increase in the times of vibration of the glass and copper needles, when exposed to the sun above those when screened, is to be attributed to the slight expansion of the needles themselves, and perhaps of the suspending wires; but this is too small at all to account for the terminal excess with these needles. I am aware that, had the copper needle been of the same weight as the magnetised needle, and the times of vibration of all the needles more nearly the same, in the series of experiments which I have given, the results would have been more strictly comparative; but on the only day during which to the present time the sun has continued unclouded long enough for making a complete series with different needles, I was unprovided with any lighter copper; and since I have formed needles of magnetised and unmagnetised sheets of copper and of glass, all of the same form and weight, and adjusted the suspending wire so that they shall vibrate in the same time, I have in vain made almost daily attempts to obtain a complete set of observations. The observations however which I have given are, I think, quite conclusive as to the fact of the compound solar rays being possessed of magnetic properties.

As we can scarcely suppose that the terminal excess in the case of the glass needle is due to magnetism in the glass, deducting this portion from the terminal excess with the other needles, the ratio of this excess with the magnetised needle to that with the copper needle would be 17 to 1, a result not very improbable, though the small difference in this excess with the copper and glass needles may very possibly have arisen from other causes. But if the effect with the glass needle cannot be attributed to magnetic influence any how exerted, what cause are we to assign for it? It is possible that the terminal excess in this case may have arisen from currents of air excited within the box by the rays of the sun: or probably electricity is excited in the glass needle on exposure to the rays of the sun, and the box becoming electric by induction, a drag between the needle and the box causes a diminution in the arc of vibration. The latter supposition would accord with the recent highly interesting experiments of Mr. BABBAGE. I am not aware that such effects as these have ever before been observed during vibration, and there certainly appears great difficulty in assigning any cause for them: however, until further observations may have thrown greater light on the subject, we must be satisfied with having ascertained the facts.

I have not as yet made any observations to ascertain what may be the effects when a needle vibrates exposed to the separated rays, but having an apparatus for the purpose, I propose doing so when the season becomes more favourable for such experiments than it is at present.

Royal Military Academy,
20th April, 1826.

XVII. *On the mutual action of sulphuric acid and alcohol, with observations on the composition and properties of the resulting compound. By Mr.* HENRY HENNELL, *chemical operator at Apothecaries' Hall. Communicated by* W. T. BRANDE, *Esq. Sec. R. S.*

Read March 9, 1826.

THE following experiments were originally undertaken with the view of ascertaining the nature of that singular product of the distillation of sulphuric acid and alcohol, which has long been known in the pharmaceutical laboratory under the name of oil of wine, and which has generally been regarded by chemists as a modification of sulphuric ether. The results however of my enquiries have led me to very different conclusions, and induce me to regard it as a hitherto undescribed compound of sulphuric acid and carbon and hydrogen; the latter elements existing in the same proportions as in olefiant gas, and exerting a peculiar saturating power in respect to the acid. I have also ascertained that hydrocarbon, with an additional proportion of sulphuric acid, affords a compound which is capable of uniting with salifiable bases, and of forming a distinct series of products.

Of oil of wine.

As I originally considered the elements of oil of wine to be the same as those of ether, I endeavoured to ascertain their relative proportions by passing its vapour over red hot oxide of copper in a glass tube, in the apparatus contrived for such decompositions by Dr. PROUT and Mr. COOPER, Trans. Soc. Arts XLI. p. 56. In these experiments I always obtained, along with the other products, a considerable proportion of sulphurous acid, and afterwards upon washing the contents of the tube with water upon a filter, it was of a bluish tint, and held sulphate of copper in solution; a result which I could not readily account for, as every precaution had been taken to free the oil of wine which I used from all adhering sulphurous and sulphuric acid.

I now added a few drops of the same oil of wine to a solution of muriate of baryta and gently heated the mixture, when not the slightest cloudiness was produced, although litmus paper indicated the existence of a free acid; but upon evaporating the mixture a precipitate fell, when it became concentrated, and on boiling it to dryness, a considerable quantity of sulphate of baryta was found in the residue: it became evident therefore that the sulphuric acid was in some state of combination which prevented its usual action upon tests, or that its elements were in some peculiar state of arrangement in the oil of wine.

To determine the quantity of sulphuric acid thus elicited, I boiled 200 grains of very carefully prepared oil of wine, free from all trace of acid, with a solution of caustic potassa to dryness; the residue was heated red hot and dissolved in water, the excess of potash being slightly supersaturated with dilute nitric acid; muriate of baryta was then added as long as it formed a precipitate, and 218,3 grains of sulphate of baryta were thus obtained. A repetition of this experiment gave the same results; so that we may conclude upon the presence of 74 grains of sulphuric acid in 200 of oil of wine.

In resuming the analysis of oil of wine by ignited oxide of copper, I found it necessary to mix it perfectly with the greater part of the oxide employed, otherwise, as in the first experiment, suphurous acid was formed in consequence of the perfect reduction of a portion of oxide, and the action of the metal thus reduced upon the sulphuric acid. With this precaution several experiments were performed, the results of which were very uniform, and as follows: 2.08 grains of oil of wine carefully freed from all adhering moisture by quicklime, were properly mixed with 200 grains of oxide of copper, and subjected with due precautions to a red heat, in the apparatus formerly adverted to: the products were 8.8 cubic inches of carbonic acid gas, and 1,54 grains of water, and these are equivalent to 0,171 of a grain of hydrogen, and 1,118 grains of carbon: 100 grains therefore of oil of wine would afford

Hydrogen	8,30
Carbon -	53,70
	62.

and the deficiency of 38 grains must be referred to sulphuric acid, a conclusion which is verified by the former experiment with solution of potassa, in which the proportion of that acid is shown to be 37 per cent. We may I think therefore conclude the above estimate to be near the truth, and the results, as respects the carbon and hydrogen, approximate nearly to the proportional quantities 6 and 1.

From the above experiment however we can only infer the composition of the hydrocarbon, which is combined with and neutralises the sulphuric acid, for in all the specimens of oil of wine which I have examined, I have found a variable quantity of hydrocarbon held in solution, part of which spontaneously separates in a crystalline form when it has been kept for some time, or when exposed to cold, but the whole of which I have not yet devised any means of separating; this dissolved hydro-carbon, however, as future experiments will show, appears to be composed of single proportionals of its elements, we must therefore have recourse to other experiments to determine the actual weight of hydrocarbon in its neutral or atomic compound with sulphuric acid.

Having thus far made out the composition of oil of wine, I examined more carefully what had taken place during its action upon heated solutions of muriate of baryta and potash; in which case, as I have already observed, an acid had been formed not capable of precipitating baryta.

200 grains of oil of wine were placed in a flask with 5 or 6 ounces of water, and the flask set in a vessel of boiling water for an hour; precipitated carbonate of baryta was then added, and immediately dissolved with effervescence; about 90 grains of carbonate were required to neutralize the acid formed: the solution filtered and set to evaporate soon became acid, and sulphate of baryta precipitated. 200 grains more of oil of wine were treated in the same way, but instead of evaporating the baryta solution, it was precipitated by carbonate of potash; the potash solution evaporated at a temperature of 150° until it crystallized, remained perfectly neutral; the crystals were thin plates, not unlike chlorate of potash, greasy to the touch, very soluble in water and alcohol, burning when heated with a flame like that of ether, and leaving an acid sulphate of potash. A few grains of these

crystals were heated in a tube, when they fused, swelled up, and gave off a dense white vapour, which condensed into an oil-like fluid, smelling strongly of sulphurous acid; the residuary salt was an acid sulphate of potash.

The following experiments were now undertaken with a view of more accurately determining the composition of this crystalline salt.

20 grains of the crystals being heated to redness, left 10,56 grains of sulphate of potassa, equal to 4,8 sulphuric acid, 5,76 potash. 20 grains were dissolved in a solution of caustic potash, boiled to dryness, heated red hot, when cold dissolved in distilled water, the excess of potash saturated by nitric acid, and the solution added to one of muriate of baryta; 28 grains of sulphate of baryta were obtained, very nearly equal to 9,6 sulphuric acid; the salt therefore contained twice the quantity of sulphuric acid required to form a neutral sulphate with the potash, or two proportionals.

In order to ascertain the proportions of the remaining elements of the salt, 5 grains were heated with oxide of copper, 5,5 cubic inches of carbonic acid gas, and 1,4 grains of water were collected; several of these experiments were made with similar results; 20 grains of the salt had been found to contain 5,76 potash,
9,60 sulphuric acid.

5 grains therefore must have contained	1,44 potash.
	2,40 sulphuric acid.
5,5 cubic inches carbonic acid gas contain	0,699 carbon.
The water obtained was - - -	1,40.
	5,939 grains.

making an excess over the 5 grains employed of 939 of a grain. If this excess be oxygen furnished to hydrogen to form part of the water obtained, and such a view is confirmed by the loss of weight of the tube and its contents after the operation, it will give of hydrogen 0,1174 of a grain, and of water so formed 1,05 grains; this deducted from the whole quantity of water obtained, leaves 0,35 of a grain water of crystallization; 100 grains would therefore be composed of

Potash	-	28,84
Sulphuric acid		48,84
Carbon	-	13,98
Hydrogen	-	2,34
Water	-	7.

These numbers indicate nearly one proportional of potash, two of sulphuric acid, four of carbon and four of hydrogen; and it would appear that in these salts the four proportionals of carbon, with the four of hydrogen, are saturating one of sulphuric acid. I am not able at present to account for the difference between the quantity of water and a proportional number, every precaution having been taken in these experiments to insure accuracy. Several attempts were made to procure an anhydrous salt, but without success, in consequence of the facility with which these acids and other compounds decompose.

The resemblance of these salts to the sulphovinates, induced me to suppose they might be similar in composition, I therefore prepared some sulphovinate of potash. Its crystalline form was the same as that of the salt obtained from oil of wine, and upon examination it proved in all respects similar.

While preparing some of the sulphovinates, I was struck with the very great change produced in sulphuric acid by mere mixture with alcohol.

440 grains of sulphuric acid were mixed with an equal weight of alcohol of specific gravity ,820 ; the mixture when cold was diluted with water and saturated by carbonate of soda, partially dried, of which it required for saturation 398 grains, while 440 grains of sulphuric acid not mixed with alcohol saturated 555 grains of the same carbonate of soda, so that $\frac{2}{7}$ of the acid had been saturated by the alcohol.

440 grains of sulphuric acid mixed with its own weight of alcohol, as before, and then poured into a solution of acetate of lead, 542 grains of sulphate were precipitated. The same quantity of sulphuric acid unchanged by alcohol gave 1313 grains of sulphate of lead; thus $\frac{4}{7}$ of the sulphuric acid had lost its power of precipitating oxide of lead from its solutions; it had in fact been converted into sulphovinic acid.

M. VOGEL, who has particularly described some of these salts, and I believe also M. GAY LUSSAC, have supposed that this loss of saturating power arises from the formation of hyposulphuric acid, and that the hyposulphates, and sulphovinates, only differ in the latter containing some ethereal oil, which in some way acts the part of water of crystallization. It is evident that the properties of oil of wine cannot be thus explained; and it appears to me more probable that the power of combination which hydrocarbon is shown to be possessed of in oil of wine, is effective in neutralising half the acid of the salts formed from it, as before described.

It only now remains to examine the hydrocarbon in the states in which it has been obtained separate from its combinations.

When oil of wine is heated in a solution of potash, or if heated in water merely, the excess of hydrocarbon above that necessary to constitute the acid, forming the salts I have described, is liberated in the form of an oil, not unlike in appearance castor oil, having but little fluidity when cold, and in some cases partially crystallizing. When gently heated it is beautifully bright, and of an amber colour; the vapour has an agreeable pungent and aromatic smell; it evaporates at a temperature a little above that of boiling water; burns with a brilliant flame, throwing off some carbon; its specific gravity is about 9, water being 10; it is insoluble in water, very soluble in ether, somewhat less so in alcohol.

Several analytical experiments were made upon this substance with similar results. When decomposed by oxide of copper, 0,72 of a grain gave 4,85 cubic inches of carbonic acid gas and ,85 of a grain of water; 4,85 cubic inches of carbonic acid gas are equal to 0,6164 of a grain of carbon, and the 0,85 of a grain of water to 0,09444 of a grain of hydrogen; 100 parts should therefore be composed of

Carbon	85,61
Hydrogen	13,116.

There is here some trifling loss; if that be supposed to be hydrogen, this oily matter is precisely similar in the proportions of its elements to olefiant gas.

The crystals which spontaneously separate from oil of wine were next examined; they were prismatic, and resembled precisely in all their characters, except their solid form, the fluid substance just described. They fused at a temperature a little above that of boiling water. After purifying a portion by pressing them in blotting paper, to remove any adhering oil of wine, several experiments were made upon quantities of a grain each; 6,46 cubic inches of carbonic acid gas and 1,21 grains of water were obtained; the 6,46 cubic inches of carbonic acid equal ,82106 of a grain of carbon, and the 1,21 grains of water equal ,13444 of a grain of hydrogen.

The carbon and hydrogen are here very nearly in single proportionals, but there is great loss, I believe: this may be partly attributed to oil of wine still adhering, but of that I am not at present able to satisfy myself, my stock of crystals being exhausted.

On mentioning these results to Mr. Faraday, he gave me some sulphuric acid which had been exposed to olefiant gas, during some of his experiments on the products of the decomposition of oil by heat. It had absorbed about 80 times its volume of the gas, acquired a deep brown colour, and a smell resembling oil of wine. It was saturated by carbonate of potash carefully evaporated to dryness, and the dry mass digested in alcohol. A small quantity of a salt was obtained from the alcoholic solution having the crystalline form and general characters of the salts I have been describing.

Thus it would appear that hydrocarbon constituted of single proportionals, or 6 carbon and 1 hydrogen by weight has the power of combining with sulphuric acid; and that whether it be evolved and then combined, as in the case of olefiant gas, or its elements separated from other compounds, as from alcohol, it forms precisely the same combination, sulphovinic acid. It further appears, that oil of wine is a perfectly neutral compound of sulphuric acid and hydrocarbon, and that it is resolvable by various processes into sulphovinic acid, during which it loses hydrocarbon, and acquires a saturating power equivalent to only half the natural power of the sulphuric acid it contains. The remaining hydrocarbon enters with it into saline combinations, and is there equivalent in saturating power to the quantity of base taken up. The proportions of hydrocarbon which in this way replaces bases being four proportionals, or 24 carbon, 4 hydrogen.

XVIII. *On a method of expressing by signs the action of machinery. By* Charles Babbage, *Esq. F. R. S. Communicated January* 17, 1826.

Read March 16, 1826.

In the construction of an engine, on which I have now been for some time occupied, for the purpose of calculating tables and impressing the results on plates of copper, I experienced great delay and inconvenience from the difficulty of ascertaining from the drawings the state of motion or rest of any individual part at any given instant of time: and if it became necessary to enquire into the state of several parts at the same moment the labour was much encreased.

In the description of machinery by means of drawings, it is only possible to represent an engine in one particular state of its action. If indeed it is very simple in its operations, a succession of drawings may be made of it in each state of its progress which will represent its whole course; but this rarely happens, and is attended with the inconvenience and expence of numerous drawings. The difficulty of retaining in the mind all the cotemporaneous and successive movements of a complicated machine, and the still greater difficulty of properly timing movements which had already been provided for, induced me to seek for some method by which I might at a glance of the eye select any particular part, and find at any given time its state of motion or rest, its relation to the motions of any other part of the machine, and if necessary trace back the sources of its movement through all its successive stages to the original moving power. I soon felt that the forms of ordinary language were far too diffuse to admit of any expectation of removing the difficulty, and being convinced from experience of the vast power which analysis derives from the great condensation of meaning in the language it employs, I was not long in deciding that the most favourable path to pursue was to have recourse to the language of signs. It then became necessary to contrive a notation which ought if possible to be at once simple and expressive, easily understood at the commencement, and capable of being readily retained in the memory from the proper adaptation of the signs to the circumstances they were intended to represent. The first thing to be done was obviously to make an accurate enumeration of all the moving parts, and to appropriate a name to each; the multitude of different contrivances in various machinery, precluded all idea of substituting signs for these parts. They were therefore written down in succession, only observing to preserve such an order that those which jointly concur for accomplishing the effect of any separate part of the machine might be found situated near to each other: thus in a clock, those parts which belong to the striking part ought to be placed together, whilst those by which the repeating part operates ought, although kept distinct, yet to be as a whole, adjacent to the former part.

Each of these names is attached to a faint line which runs longitudinally down the page, and which may for the sake of reference be called its *indicating line.*

The next object was to connect the notation with the drawings of the machine, in order that the two might mutually illustrate and explain each other.

It is convenient in the three representations of a machine, to employ the same letters for each part; in order to connect these with the notations, the letters which in the several drawings refer to the same parts, are placed upon the indicating lines immediately under the names of the things. If circumstances should prevent us from adhering to this rule, it would be desirable to mark those things represented in the plan by the ordinary letters of the alphabet, those pointed out for one of the other projections by the letters of an accented alphabet, and the parts delineated on the third projection by a doubly accented alphabet. In engines of so complicated a nature as to require sections at various parts as well as the three projections, this system is equally applicable, and its advantage consists in this—that the number of accents on the letter indicates at once the number of the drawing on which it appears, and when it is intended to refer to several at the same time, the requisite letters may be employed and placed in the order in which the drawings will best illustrate the part under examination.

The next circumstance which can be indicated by the system of mechanical notation which I propose, more readily than by drawings, is the number of teeth on each wheel or sector, or the number of pins or studs on any revolving barrel. A line immediately succeeding that which contains the references to the drawings is devoted to this purpose, and on each vertical line indicating any particular part of the machine, is written the number of teeth belonging to it. As there is generally a great variety of parts of machinery which do not consist of teeth; of course every vertical line will not have a number attached to it.

The three lines immediately succeeding this, are devoted to the indication of the velocities of the several parts of the machine. The first must have, on the indicating line of all those parts which have a rectilinear motion, numbers expressing the velocity with which those parts move, and if this velocity is variable, two numbers should be written, one expressing the greatest, the other the least velocity of the part. The second line must have numbers expressing the angular velocity of all those parts which revolve; the time of revoluion of some one of them being taken as the unit of the measure of angular velocity.

It sometimes happens that two wheels have the same angular velocity when they move; but from the structure of the machine, one of them rests one half of the time during which the other is in action. In this case, although their angular velocities are equal, their comparative velocities are as 1 to 2; for the second wheel makes two revolutions, whilst the other only makes one. A line is devoted to the numbers which thus arise, and it is entitled, Comparative Angular Velocity.

The next object to be considered is the course through which the moving power is transmitted, and the particular modes by which each part derives its movement from that immediately preceding it in the order of action. The sign which I have chosen to indicate this transmission of motion (an arrow), is one very generally employed to denote the direction of motion in mechanical drawings; it will therefore readily suggest the direction in which the movement is transmitted. There are however various ways by which motion is communicated; and it becomes a matter of some importance to consider whether, without interfering with the sign just selected, some modification might not be introduced into its minor parts, which, leaving it unaltered in the general form, should yet indicate the peculiar nature of the means by which the movement is accomplished.

On enumerating those modes in which motion is usually communicated, it appeared that they may be reduced to the following.

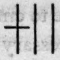
One piece may receive its motion from another by being permanently attached to it as a pin on a wheel, or a wheel and pinion on the same axis.	**This may be indicated by an arrow with a bar at the end.** ⊢⟶
One piece may be driven by another in such a manner that when the driver moves, the other also always moves; as happens when a wheel is driven by a pinion.	**An arrow without any bar.** ⟶
One thing may be attached to another by stiff friction.	**An arrow formed of a line interrupted by dots.** -·-·-·->
One piece may be driven by another, and yet not always move when the latter moves; as is the case when a stud lifts a bolt once in the course of its revolution.	**By an arrow, the first half of which is a full line, and the second half a dotted one.** ——·······>
One wheel may be connected with another by a ratchet, as the great wheel of a clock is attached to the fusee.	**By a dotted arrow with a ratchet tooth at its end.** ·↖·········>

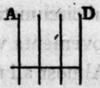
Each of the vertical lines, representing any part of the machine, must now be connected with that representing the part from which it receives its movement, by an arrow of such a kind as the preceding table indicates; and if any part derives motion from two or more sources, it must be connected by the proper arrows with each origin of its movement. It will in some cases contribute to the better understanding of the machine, if those parts which derive movement from two or more sources have their names connected by a bracket, with two or more vertical lines, which may be employed to indicate the different motions separately. Thus if a shaft has a circular as well as a longitudinal motion, the two lines attached to its name should be characterised by a distinguishing mark, such as (vert. motion) and (circ. motion). Whenever any two or more motions take place at the same time, this is essential; and when they do not, it is convenient for the purpose of distinguishing them.

All machines require, after their parts are finished and put together, certain alterations which are called adjustments. Some of these are permanent, and, when once fixed by the maker, require no further care. Others depend on the nature of the work they are intended to perform, as in the instance of a corn mill; the distance between the stones is altered according to the fineness of the flour to be ground: these may be called usual adjustments; whilst there are others depending on the winding up of a weight, or spring, which may be called periodic adjustments. As it is very desirable to know all the adjustments of a machine, a space is reserved, below that in which the connections of the moving parts are exhibited, where these may be indicated; if there are many adjustments, this space may be subdivided into three, and appropriated to each of the three species just enumerated, the permanent, the usual, and the periodic: if their number is small, it is better merely to distinguish them by a modification of their signs. It is sometimes impossible to perform such adjustment, except in a particular succession; and it is always convenient to adhere to one particular order. Numbers attached to their respective lines denote the order in which the parts are to be adjusted; and as it will sometimes happen that two or more adjustments must be made at the same time, in that case the same numbers must be written on the lines belonging to all those parts which it is necessary to adjust simultaneously.

If it is convenient to distinguish between the species of adjustments without separating them by lines, this may be accomplished by putting a line above or below the figures, or inclosing them in a circle, or by some similar mode. I have attached the letter P to those which are periodic.

It would add to the knowlege thus conveyed, if the sign indicating adjustment also gave us some information respecting its nature; there are, however, so many different species, that it is perhaps better in the first instance to confine ourselves to a few of the most common, and to leave to those whom may have occasion to employ this kind of notation, the contrivance of signs, fitted for their more immediate purpose.

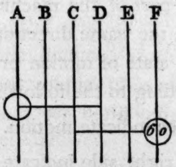
One of the most common adjustments is that of determining the distance between two parts, as between the point of suspension and the centre of oscillation of a pendulum. This might be indicated by a small line crossing the vertical line attached to the part.  If the distance between two parts, which are represented by different lines, is to be altered, their lines may be connected by an horizontal line.



Thus the adjustment of A in the above figure depends on its distance from D.

This adjustment often determines the length of a stroke, and sometimes the eccentricity of an eccentric, which depends on the linear distance between its centre and its centre of motion.

The next most frequently occurring adjustment is that which is sometimes necessary in fixing two wheels, or a wheel and an arm on the same axis. This relation of angular position, may be indicated by a circle and two radii placed at the requisite angle, or that angle may be stated in figures and inclosed within the circle; this circle ought however to be connected by a line with the other part, with which the angle is to be formed, thus,



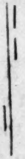
which means that an adjustment is to be made by fixing A on its axis, making a right angle with D, and that F must also make with C an angle of sixty degrees. In speaking of these angles, it should always be observed that they refer to the angles made by the parts on one of the planes of projection.

When it is thought requisite to enter into this minute detail of adjustments, it will be necessary, in order to avoid confusion, to put the lines indicating the order of adjustment, above and distinct from these signs.

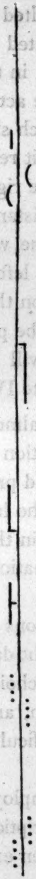
The last and most essential circumstance to be represented, is the succession of the movements which take place in the working of the machine. Almost all machinery, after a certain number of successive operations, recommences the same course which it had just completed, and the work which it performs usually consists of a multitude of repetitions of the same course of particular motions.

It is one of the great objects of the notation I am now explaining, to point out a method by which, at any instant of time in this course or cycle of operations of any machine, we may know the state of motion or rest of every particular part; to present a picture by which we may, on inspection, see not only the motion at that moment of time, but the whole history of its movements, as well as that of all the cotemporaneous changes from the beginning of the cycle.

In order to accomplish this, each of the vertical indicating lines representing any part of the machine, has, adjacent to it, other lines drawn in the same direction: these accompanying lines denote the state of motion or rest of the part to which they refer, according to the following rules.



1. Unbroken lines indicate motion.
2. Lines on the right side indicate that the motion is from right to left.
3. Lines on the left side indicate that the direction of the motion is from left to right.



4. If the movements are such as not to admit of this distinction, then when lines are drawn adjacent to an indicating line, and on opposite sides of it, they signify motions in opposite directions.

5. Parallel straight lines denote uniform motion.

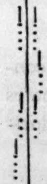
6. Curved lines denote a variable velocity. It is convenient as far as possible to make the ordinates of the curve proportional to the different velocities.

7. If the motion may be greater or less within certain limits: then if the motion begin at a fixed moment of time, and it is uncertain when it will terminate, the line denoting motion must extend from one limit to the other, and must be connected by a small cross line at its commencement with the indicating line. If the beginning of its motion is uncertain, but its end determined, then the cross line must be at its termination. If the commencement and the termination of any motion are both uncertain, the line representing motion must be connected with the indicating line in the middle by a cross line.

8. Dotted lines imply rest. It is convenient sometimes to denote a state of rest by the absence of any line whatever.

9. If the thing indicated be a click, bolt, or valve, its dotted line should be on the right side if it is out of action, unbolted, or open, and on the left side if the reverse is the case.

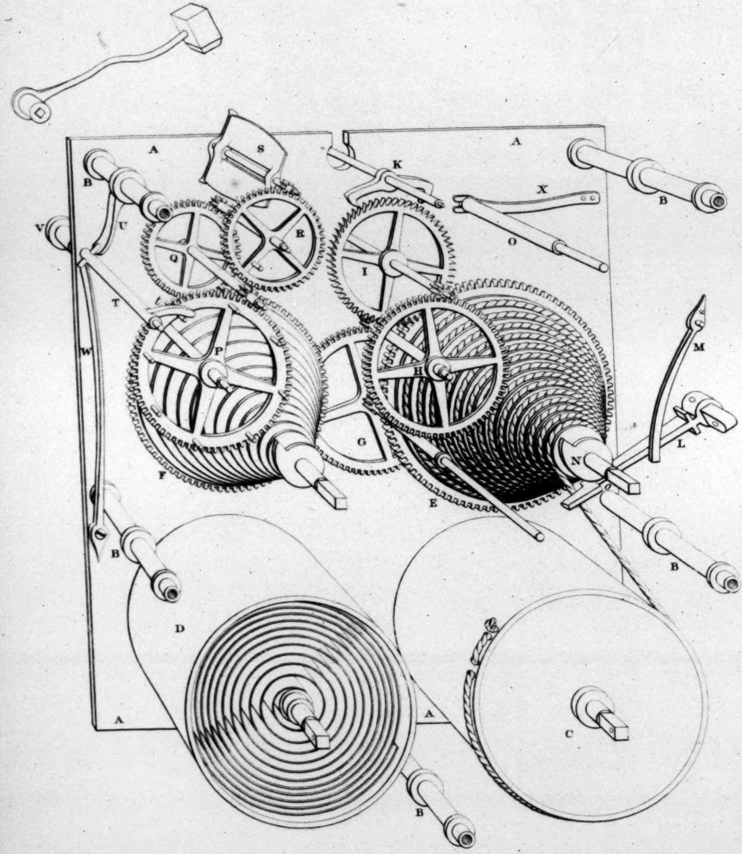
10. If a bolt may rest in three positions: 1st, bolted on the right side; 2nd, unbolted; 3rd, bolted on the left side. When it is unbolted, and in the middle station, use two lines whilst in the act of unbolting, and two lines of dots, one on each side of, and close to the indicating line, whilst it rests in this position. When it is bolted on the right side, a line or a line of dots at a greater distance on the right hand from the indicating line will represent it. And if it is bolted on the left, a similar mode of denoting it must be used on that side. Any explanation may, if required, be put in words at the end of the notation, as will be observed in that of the hydraulic ram, Plate IV.



I have now explained means of denoting by signs almost all those circumstances which usually occur in the motion of machinery: if other modifications of movement should present themselves, it will not be difficult for any one who has rendered himself familiar with the symbols employed in this Paper, to contrive others adapted to the new combinations which may present themselves.

The two machines which I have selected as illustrations of the application of this method, are, the common eight day clock, and the hydraulic ram. The former was made choice of from its construction being very generally known, and I was induced to choose the latter from the apparent difficulty of applying this method to its operations.

The advantages which appear to result from the employment of this *mechanical notation*, are to render the description of machinery considerably shorter than it can be when ex-



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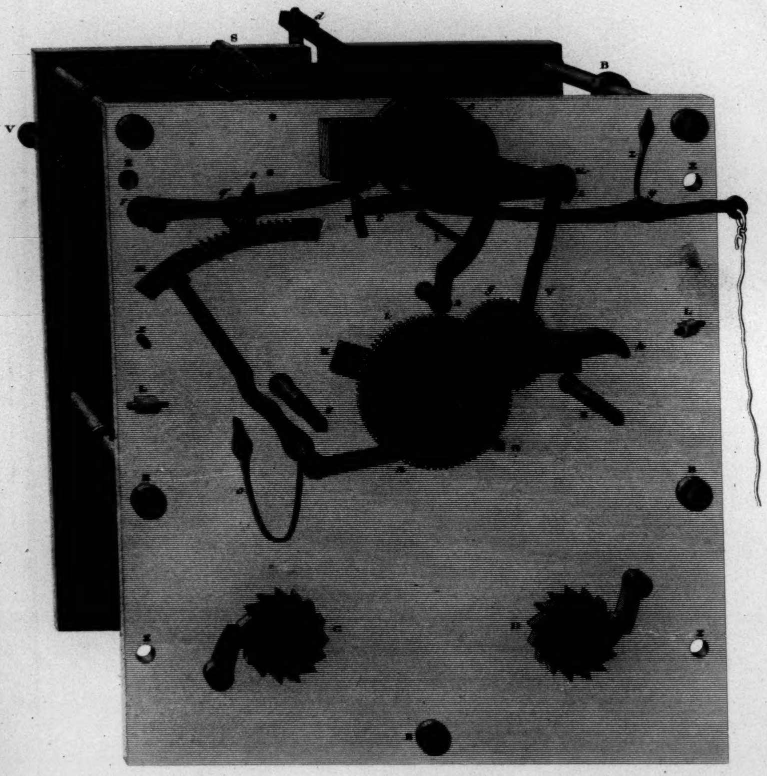
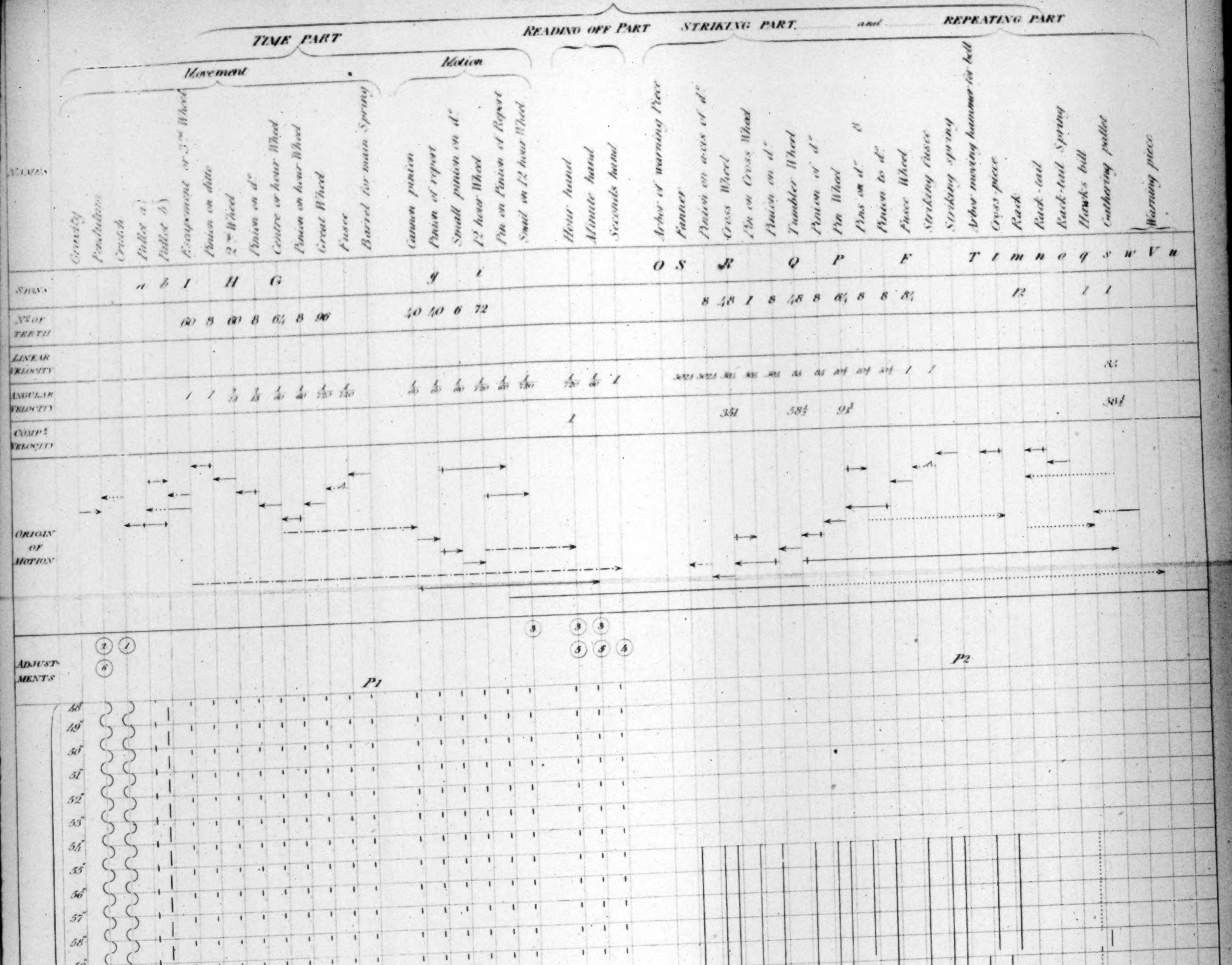
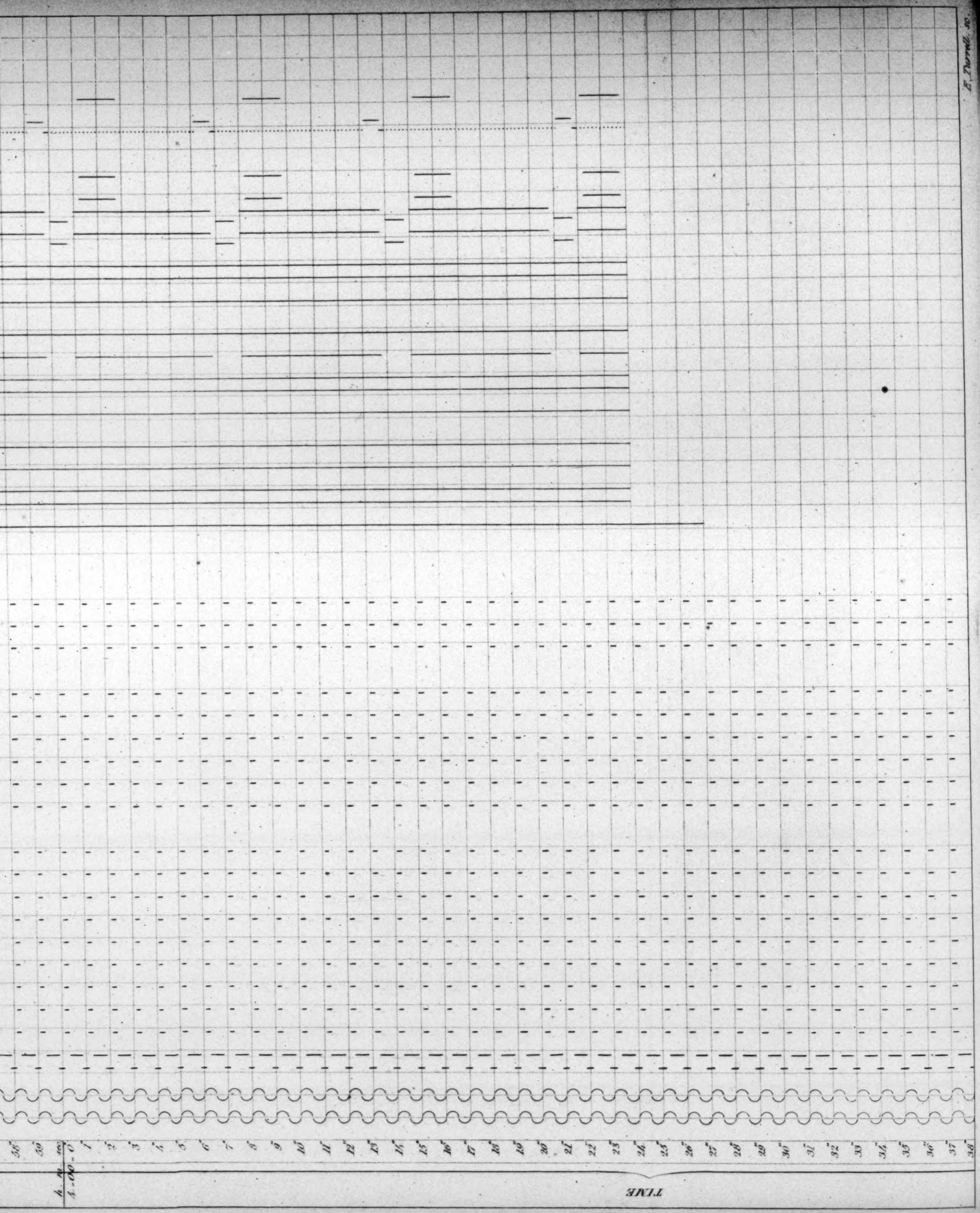


P. Turrell. sculp.

EIGHT DAY CLOCK.



58
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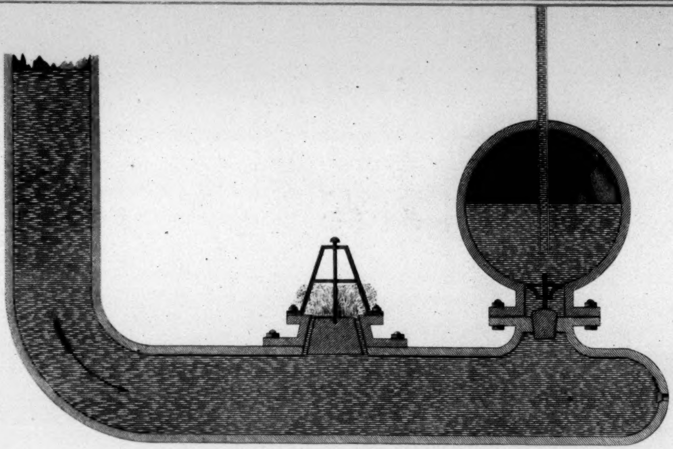
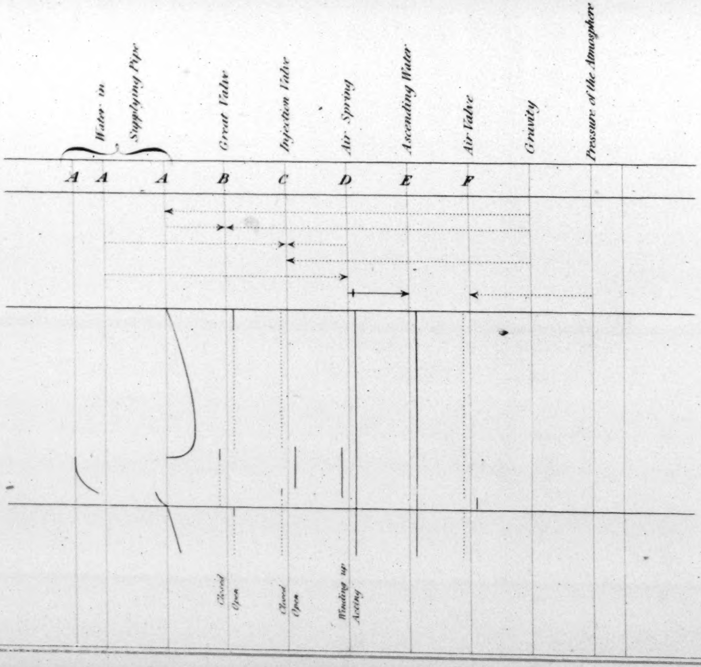
TIME
E. Turrell, sc.

Phil. Trans. MDCCCXXVI. Plate. X. p. 261.





E. Turrell. sc.

pressed in words. The signs, if they have been properly chosen, and if they should be generally adopted, will form as it were an universal language; and to those who become skilful in their use, they will supply the means of writing down at sight even the most complicated machine, and of understanding the order and succession of the movements of any engine of which they possess the drawings and the mechanical notation. In contriving machinery, in which it is necessary that numerous wheels and levers, deriving their motion from distant parts of the engine, should concur at some instant of time, or in some precise order, for the proper performance of a particular operation, it furnishes most important assistance; and I have myself experienced the advantages of its application to my own calculating engine, when all other methods appeared nearly hopeless.

DESCRIPTION OF THE PLATES.

Plates VII. and VIII. are different representations of an eight day clock, for the purpose of comparing it with the notation.

Plate IX. represents the mechanical notation of the same clock.

Under the names of each part follow the letters which distinguish them in the plates.

The next line contains numbers which mark the number of teeth in each wheel, pinion, or sector.

The following line is intended to contain numbers expressing the linear velocity of the different parts: in the eight day clock this line is vacant, because almost all the motions are circular.

The next line indicates the angular velocity of each part;

and in order to render the velocity of the striking parts comparable with those of the time part, I have supposed one revolution of the striking fusee to be made in one minute: I have also taken one revolution of the scapement wheel, or one minute of time as the unit of angular velocity.

The space entitled comparative angular velocity, expresses the number of revolutions one wheel makes during one revolution of some other; it differs from the real angular velocity, because one wheel may be at rest during part of the time. Thus the clock strikes 78 strokes during twelve hours, or one revolution of the hour hand: if this be called unity, the pin wheel moves through 78 pins or $9\frac{3}{4}$ revolutions in the same time; its comparative angular velocity is therefore $9\frac{3}{4}$.

The space in which the origin of motion is given, will not require any explanation after reading the description of the signs employed in this paper.

The adjustments are numbered in the order in which they are to be made. No. 1, is attached to the crutch: the first adjustment is to set the clock in beat. No. 2, is to adjust the length of the pendulum to beat seconds. No. 3, occurs in three different places, at the hour and minute hands, and at the snail on the hour wheel. It is necessary that when the hour hand is at a given figure, three o'clock for instance, that the minute hand should be set to twelve o'clock; it is also necessary that the snail should be in such a position that the clock may strike three. These adjustments must be made at the same time. No. 4, is for the adjustment of the seconds hand to 60 seconds. No. 5, is double, and is for the adjustment of the minute and hour hand to the next whole minute to that which is indicated by the watch by which the clock is set. No. 6, is for the pendulum, which muſt be held aside at its extreme arc until the instant at which the watch reaches the time set on the face of the clock; it muſt then be set free.

The remaining part of the notation indicates the action of every part at all times; but as the whole cycle of twelve hours would occupy too much space, a portion only is given about the hour of four: from this the machine may be sufficiently understood. As an instance of its use, let us enquire what movements are taking place at seven seconds after four o'clock. On looking down on the left hand side to the time just mentioned, we observe between the end of the sixth, and end of the seventh second, that the pendulum and crutch begin to move from the right to the left, increasing their velocity to a maximum, and then diminishing it; that the whole train of wheels of the time part are at rest during the greater part of that second, and all move simultaneously a little before its termination. The greater part of the train of the striking part is moving uniformly; but two parts the cross piece, and the other moving the hammer, being at the commencement of this second in a state of motion from right to left, suddenly have that motion reversed for a short time: this is at the moment of striking: two other pieces, the hawk's bill, and the gathering pallet, appears to act at the same moment.

If the course of movement of any one part is required throughout the whole cycle of the machine's action, we have only to follow its indicating line. If it is required to find what motions take place at the same time, we have only to look along the horizontal line marked by the time specified.

Let us now enquire into the source of motion of the minute hand. On looking down to the space in which the origin of motion is given, we observe an arrow point, which conveys us to the

Cannon pinion, with which it is connected permanently.

The cannon pinion is driven by the centre or hour wheel, with which it is connected by stiff friction.

The hour wheel is driven by its pinion, to which it is permanently attached.

The hour wheel pinion is driven by the great wheel, into which it works.

The great wheel is driven by the fusee, with which it is connected by a ratchet.

The fusee is driven by the spring barrel or main spring, which is the origin of all the movements.

When that part of the notation which relates to the successive movements of the machine is of considerable extent, it is convenient to write on a separate piece of paper the names of every part, at the same distances from each other as the indicating lines, and exactly as they are placed at the top. By sliding this paper down the page to any part which is under consideration, the trouble of continual reference to the top of the drawing will be avoided.

Plate X. represents the hydraulic ram; its mechanical notation is added below it.

A, is the supplying pipe.
B, is the great valve.
C, the valve into the air vessel.
D, the air vessel.
E, the ascending water.
F, the small air valve:—its office is to supply a small

quantity of air at each stroke; it opens when the valve C is just closed, and a regurgitation takes place in the supplying pipe just previous to the opening of the great valve. Without this contrivance, the pressure on the air in the air vessel would cause it to be soon absorbed by the water, and the engine would cease to act.

In this notation two indicating lines A, A, are allowed to the supplying water, because it takes three different courses during the action of the machine. The first of these marks the time of its motion when it enters the air vessel, and the second indicates its course when passing through the great valve, and also its course when, owing to the elasticity of the materials, its motion is for an instant reversed, at which moment air is taken in at the air valve F.

The action of the machine is as follows: the supplying water rushing along the great pipe passes out at the great valve; it acquires velocity until the pressure of the effluent water against the under part of the great valve causes it to close suddenly. At this moment the whole momentum of the water is directed against the sides of the machine, and the air valve being the weakest part gives way, and admits a small quantity of water; the air spring soon resists sufficiently to close the air valve: at this moment the elasticity of the apparatus re-acting on the water in the great pipe, drives it back for an instant, during which the pressure of the atmosphere opens the air valve, and a small quantity of air enters; this finds its way to the air chambers, which easily discharges it through the ascending pipe if too much air has entered.

XIX. *On the parallax of the fixed stars. By* J. F. W. Herschel, *Esq. M. A. Sec. R. S. Communicated January* 19, 1826.

Read March 9 and 16, 1826.

The determination of the existence and amount of annual parallax in the fixed stars, was the object which originally drew the attention of astronomers to the examination and measurement of double stars, upon a principle suggested originally by Galileo, and improved on and more fully developed by my Father in one of his earliest communications to the Royal Society, Phil. Trans. 1782; according to which, two stars placed very nearly in a line with each other and with our system, ought, if situated at very different distances, in the line of sight, to be subject to periodical variations in their apparent distance from each other, according as the earth in its annual motion approaches to or recedes from that point in the plane of the ecliptic, where the line joining them, prolonged, would meet it. The difficulty of determining the distance of two stars of a double star with the necessary degree of exactness for this delicate purpose, has however hitherto put a stop to this enquiry; and, as it is now rendered extremely probable that the parallax at least of the generality of stars is much below 1″, must continue to do so, until some very great improvement of micrometers shall enable us to measure tenths of seconds with as great certainty as we at present can units.

I do not find that it has been noticed, however, that parallax must occasion a periodical change in the angle of position, as well as in the distance of the two stars composing a double star, and that this variation is much more susceptible of ready and exact appreciation with our present micrometers than that of their distance. To render this sensible, we need only remark, that the effect of parallax is to cause each star to describe (apparently) a small ellipse in the heavens, the major axis of which is parallel to the ecliptic, and the minor in the direction of a secondary to that great circle, the true place of the stars being in the centre. Were these ellipses of the same magnitude, for each of two contiguous stars, the line joining their apparent places (which are necessarily homologous points in the circumferences of each) would preserve its parallelism at all times; but as the axes of the ellipses are reciprocally as the distances of the stars, that parallelism cannot obtain when the stars are situated at very unequal distances from the earth, and an alternate increase and decrease of the angle of position made by this line with any fixed direction must be the necessary consequence.

To estimate the extent of this variation, let us conceive two stars so situated as to have their apparent line of junction in the direction of a secondary to the ecliptic, and therefore at right angles to the major axes of their parallactic ellipses—let their distances from us be such that the nearer one shall have a parallax of 1″, and the farther no appreciable amount of it. Also, let their apparent angular distance from each other be 5″. It is evident that the variation alluded to will equal the angle subtended by a line of 1″ in length, at a point 5″ distant from its middle, that is, to 11° 25′.

Now this is a quantity which is quite beyond all conceivable limits of error of observation in the measurement of double stars, and for stars nearer than 5″ the amount is of course proportionally greater. Thus for two stars, at only 1″ distance from each other, of which the one is affected by parallax to the amount of 1″, and the other not all, the annual variation in position will amount to upwards of 53°.

When the distance between two stars amounts to no more than 3″, and in many cases even when they are still nearer, Mr. SOUTH's and my own experience in the use of the position micrometer, as well as Mr. STRUVE's numerous and excellent measures recorded in the Dorpat Observations, lead me to believe that a single degree in the angle of position is a quantity distinctly appreciable in the mean of several sets of measures carefully taken on different nights of observation, even when a considerable inequality in the stars, or other unfavourable circumstances exist; and were observations continued for a series of years at the proper times, and made with the care so delicate a subject of research would peculiarly call for, there can be no doubt that a greater degree of precision might be obtained; and it certainly seems not too much to assume, that half that quantity, or 30′ of variation in the angle of position, *if regularly periodical*, must ultimately be detected. This conclusion appears warranted by the interesting re-examination by Mr. SOUTH of stars pointed out in our joint paper on Double Stars (Ph. Tr. 1824. iii.) as having probably a relative angular motion, or as otherwise remarkable, lately communicated by him to the Royal Society, (See Part i. of the present vol.) in which we find instances of coincidence between calculated and computed motions falling within the limit assumed at the outset—too frequent, and too remarkable to be the effect of mere accident.

Now, the tangent of 30′ to a radius of 3″ corresponds to a subtense of 0".026, or $\frac{1}{40}$th of a second, so that a difference of parallaxes to the amount of a 40th of a second, existing in the two stars of a double star so circumstanced, could scarcely escape detection; and that even much less quantities than this, under favourable circumstances, might be rendered sensible, I think may fairly be concluded, when we consider that in this estimate, the data are certainly assumed within bounds. No account is here taken of the improvements in the position micrometer, which may reasonably be expected, because the object at present is only to appreciate the degree of delicacy which the method now proposed may lay claim to, with our present instruments and habits of observing, and to compare it with those which have hitherto been resorted to in the investigation of parallax.

In selecting stars for examination, it appears to me that we ought by no means to confine ourselves, by assuming it as a universal law that the brightest stars are the nearest to us. From what we know of the variety of nature and the enormous differences in point of magnitude between the bodies of our own system, it seems improbable that the real magnitude and brightness of the stars should be confined within narrow limits. Their distances are equally undetermined; nor have we any reason whatever to conceive these two elements related to each other. There is not therefore the slightest *a priori* improbability, in supposing that among stars of apparently equal lustre, the greatest diversity of distance may exist, or that innumerable of the minutest stars visible in telescopes may be nearer to us than any of those of the first magnitude; and consequently, that that delicate element in search of which astronomers have exhausted refinement, may with nearly or quite equal probability of success be sought among stars of far inferior magnitudes. The proper motions of the stars afford an argument from analogy—they bear no relation to their apparent lustre, and by far the greatest proper motions known belong to stars low in the scale of magnitudes.

If these remarks have any foundation, it must be obvious that we ought not to be deterred from the research of parallax by the smallness of the stars composing a double star, or their approach to equality. However, we should except here stars in, or very near the milky way, below the 7th or 8th magnitudes, because the probable *laminar* form of this great sidereal stratum affords a presumption almost amounting to certainty, that minuteness is *here*, *on the average*, an effect of distance. But, on the other hand, such large stars as β Orionis, which are situated in, or near the borders of the milky way, with small ones near enough to constitute them double stars, have an additional claim to examination, from the additional probability thus afforded of being favourably placed for the detection of parallax: and moreover, this consideration affords plausible grounds for a belief, that, in situations remote from the milky way, minuteness, *on the average*, is *not* the effect of distance.

It is hardly necessary to insist on the great advantages presented by the method here proposed, in its complete exemption from those instrumental errors depending on unsteadiness, erroneous graduation, and expansion, and from all that uncertainty on the score of refraction, and any doubts still remaining as to the magnitudes of the constants of aberration, nutation, &c., which so much embarrass astronomers.

A good telescope and a good micrometer are all the instruments required. This advantage is really incalculable. Instead of confining our attention to one or two principal stars, it puts an almost unlimited range of objects in our power, by enabling us to employ in this research the largest telescopes, and thus easily obtain measures of those stars, which, from their faintness, must present insuperable difficulties with instruments of ordinary apertures.

In selecting objects for examination by this method, we must be chiefly guided by their angles of position and distances. Taking such whose distances are below 15″, (which, on the supposition of $\frac{1}{2}°$ periodical variation of position, corresponds to $\frac{1}{8}$ of a second of relative parallax in stars properly situated), the angle of position ought to be such, that the line joining the two stars shall point as nearly as may be, to the pole of the ecliptic. Ten, twenty, or even thirty degrees of deviation either way from this direction, will however not materially vitiate the application of this method to stars near the ecliptic, while, for such as have considerable latitudes, proportionally greater deviations may be allowed, and within thirty degrees of the pole of the ecliptic this element is of comparatively small moment.

In general, to ascertain whether any double star is or is not favourably situated for the application of this method, we must (if we would take up the problem on strict mathematical grounds), proceed as follows:

Let l represent the longitude, and $+\lambda$ the N. latitude of the star, σ its *angle of situation*,* or the angle included at the star by great circles joining it and the north poles of the equinoctial and ecliptic respectively, σ being considered positive for stars in the western hemisphere (or that whose pole is the point ♈), and negative for stars in the eastern, whose pole is ♎. Let also ⊙ represent the sun's longitude at any time, and call a the maximum semi-annual parallax or the angle (expressed in seconds), which the radius of the earth's orbit would subtend if perpendicularly presented to an eye at the distance of the star. It is obvious then that a will represent the major semi-axis of the star's parallactic ellipse, and $a . \sin. \lambda$ the minor, so that its excentricity $= \sqrt{a^2 - a^2 . \sin. \lambda^2}$ $= a . \cos. \lambda$, which if we call $a e$, we have $e = \cos. \lambda$. The star will appear to describe this ellipse in the direction *npsf*. Its motion in it will however not be uniform, but equal areas will be described in equal times about its centre (or the star's mean place): this is evident, because the area described by the star in the parallactic ellipse round its centre is the orthographic projection on the surface of the heavens of that described by the earth round the sun in its orbit. This consideration gives us at once, the equation

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* This angle in many astronomical books is called the angle of position, but as in speaking of double stars we have always hitherto called the angle made by the

$$\tan. \theta = - \sin. l . \text{cotan.} (\odot - l); \qquad (1)$$

where θ represents the elongation of the star in its ellipse from the eastern extremity of its major axis reckoned in the direction *npsf*.

Let π be the angle of position of the small star, π being reckoned from a parallel to the equinoctial, in the same direction *npsf* and from the east, so that the *nf* quadrant shall

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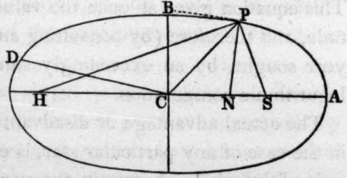
line joining the two stars with the parallel, by this name, it becomes necessary to distinguish them, and the expression used in the text is perhaps also more correct than that in common use.

correspond to values of π between 0 and 90°, the np to values between 90° and 180° and so on. It is evident, then, that when the large star is in the point of its apparent orbit immediately in the line joining the mean places of both, its elongation, or the value of θ corresponding to this situation will be $\pi - \sigma$.

Since it is only the difference of parallaxes which this method can render sensible, we may suppose the small star a fixed point, and since the dimensions of the parallactic ellipse may be supposed small in comparison with the distance of the small star, two tangents drawn from the latter to the circumference of the former, and which, of course, mark the situation of the large one in its apparent orbit where parallax has the greatest effects in opposite senses on the angle of position, will nearly meet it in the two extremities of a diameter conjugate to that passing through the small star.

To determine the direction of the conjugate diameter, we must have recourse to the general equations of the ellipse from its centre with polar co-ordinates. Thus r being the length of any semi-diameter CP, whose angle of elongation from CA, is ACP$=\varphi$ and r', that of its semiconjugate CD, the angle ACD being called φ', we have, H and S being the foci, and calling CN the abscissa from the centre, x, by a property of the conic sections, SP$=a-ex=a-er\cos.\varphi$; HP$=a+ex=a+er.\cos.\varphi$; whence, SP.HP$=\text{CD}^2=r'^2=a^2-e^2r^2.\cos.\varphi^2$.



But we have also, $r^2=\frac{a^2(1-e^2)}{1-e^2.\cos.\varphi^2}$; $r'^2=\frac{a^2(1-e^2)}{1-e^2.\cos.\varphi'^2}$.

Eliminating from these three equations, r and r', we get the relation required between φ and φ', viz.

$$\cos. \varphi'^2 = \frac{\sin. \varphi^2}{1-(2-e^2)\, e^2. \cos. \varphi^2}$$

This equation will however be reduced to a much more convenient form for our present purpose by a transformation, viz.

$$\tan. \varphi'^2 = \frac{\sin. \varphi'^2}{\cos. \varphi'^2} = \frac{1-\cos. \varphi'^2}{\cos. \varphi'^2}$$

$$= \frac{(1-e^2)^2. \cos. \varphi^2}{\sin. \varphi^2}$$

that is simply

$$-\tan. \varphi' = (1-e^2). \cot. \varphi$$

the sign — being prefixed in extracting the root, became in the ellipse C P and C D lie in different quadrants. Now in the case before us we have

$$\varphi = \pi - \sigma \text{ and } \varphi' = \theta\text{; so that}$$

$$\tan. \theta = -(1-e^2). \operatorname{cotan}. (\pi - \sigma)$$

But $(1-e^2) = \sin. \lambda^2$, and by equation (1) $-\tan. \theta = \sin. l. \operatorname{cotan}. (\odot - l)$. Hence substituting, we get

$$\sin. \lambda^2. \tan. (\odot - l) = \sin. l. \tan. (\pi - \sigma); \qquad (2)$$

This equation gives at once the value of $\odot$ the sun's longitude, and therefore (by consulting an ephemeris) the time of year sought, by an exceedingly simple process adapted to logarithmic computation.

The actual advantage or disadvantage in point of situation in the case of any particular star, is expressed by the magnitude of the whole change in the angle of position produced by parallax, *i. e.* by the angle subtended by the ellipse at the small star, or, (calling the distance of the latter from its centre δ) on the original supposition of the whole effect being small, by the expression $P = \frac{2r'}{\delta}. \sin. (\varphi' - \varphi)$. Now if we substitute for r' and sin. $(\varphi' - \varphi)$ their values in terms of φ, or $\pi - \sigma$ deduced from the equations already stated, by proper eliminations, we find

$$P = \frac{2a}{\delta} \cdot \sqrt{1 - \cos.\,\lambda^2 .\ \cos.\,(\pi - \sigma)^2} \qquad (3)$$

whence the maximum effect of parallax on the angle of position of any double star is readily computed; the expression is inconvenient for logarithmic computation, but this inconvenience is obviated by the very obvious substitution

$$\cos.\,\lambda .\ \cos.\,(\pi - \sigma) = \cos.\,M;\ P = \frac{2a}{\delta}.\ \sin.\,M. \qquad (4$$

We also have

$$2a = \delta .\ \frac{P}{\sin.\,M} = \delta .\ \frac{\sin.\,P}{\sin.\,M} \qquad (5)$$

from which we may deduce the value of $2a$ the maximum of parallax when the total effect (P) on the angle of position is known.

If we take $P = 30'$ we have $2a = \delta .\ \frac{\sin.\,30'}{\sin.\,M}$ (6)

which expresses the amount of annual parallax in any proposed star which will be indicated by a periodical variation of 30′ in the angle of position. The smallness of this amount is a criterion (mathematically speaking) of the favourable or unfavourable nature of the individual star for researches of this kind.

By these theorems the proper times of year for observation, and the amount of difference of parallax, appreciable by a variation of 30′ in the angle of position, have been computed for such stars among those observed by Mr. SOUTH and myself, in the paper above alluded to, as appear favourable to the application of the common wire micrometer to this enquiry, in the method now proposed. As the number of known double stars increases, others may easily be added to the list. The Catalogue of 460 double Stars observed by

Mr. South, and lately communicated to the Royal Society, will furnish a great many; and Mr. Struve's immense collection, amassed in his reviews with the large refractor, as well as a collection of minute double stars encountered in my own 20-feet sweeps, and which will shortly be published, doubtless many more; so that it is rather intended to give the following list as a specimen of a more complete one, than as including all, or nearly all, the stars it is desirable to examine with this view. Meanwhile, to enable others not conversant with algebraic symbols to extend the list for themselves, if so inclined, I shall here set down the whole work of calculation for one star.

Calculation for 35 Piscium. R. A. $0^h\ 6^m = \alpha = 1°\ 30'$, Decl. $7°\ 49'$ N. Long. $= l = 4°\ 24'$. Lat. $= \lambda = +\ 6°\ 32'$; Angle of position $= \pi = 298°\ 4'$; Distance $= \delta = 12''.5$.

sin. obliq^{y}.	+ 9·60012	(2) tan. $(\pi - \sigma)$	− 11·09285	(1) cos. lat.	+ 9·99717
cos. R.A.	+ 9·99985	sin. long.	+ 8·88490	(2) cos. $(\pi - \sigma)$	+ 8·90574
		(add)	− 19·97775	(3) (add) cos. M	+ 8·90291
(1) ar. comp. cos. lat.	+ 0·00283	(1) 2 × log. sin. lat.	+ 8·11214	log. δ	+ 1·04798
				tan. 30′	+ 7·94086
sin. σ	+ 9·60280	(subtr.) tan. $(\odot - l)$	− 11·86561		
σ	23° 27′	$\odot - l$	270° 47′	(add)	+ 8·98884
π	298 4	l	4 24	(3) sin. M	+ 9·99861
$\pi - \sigma$	274 37	$\odot$	275 11	(subtr.) log. 2 a	+ 8·90023
		$\odot + 6^s$	95 11	2 a	0·098
		Dates by Nautical Almanack .	Dec. 27 June 27		

If we wish to avoid the computation of the latitudes and longitudes which this supposes known, they may be taken with sufficient precision from any good map, or from a large celestial globe, as exactness is not required for this purpose in these elements.

Specimen of a List of Stars

favourably situated for the Investigation of Parallax by the Method proposed in this Paper.

General Number in Messrs. H. and S's. Observations.	Stars' Name, &c.	R. A. for 1820.		Declination for 1820.			Times of the year most proper for Observation.	Amount of annual Parallax indicated by a periodical variation of 30′ in the angle of pos.
		h.	m.	°	′			
1	35 Piscium	0	6	7	49	N	June 27, December 27	0″.098
20	γ Arietis	1	44	18	25	N	January 31, August 3	.085
25	α Piscium	1	53	1	53	N	January 18, July 19	.047
38	32 Eridani	3	45	3	30	S	February 12, August 15	.071
39	ε Persei	3	46	39	29	N	February 19, August 23	.081
46	55 Eridani	4	35	9	9	S	March 6, September 8	.103
47	ω Aurigæ	4	47	37	36	N	March 5, September 7	.069
53	Rigel	5	6	8	25	S	February 25, August 29	.084
55	118 Tauri	5	18	25	0	N	March 10, September 13	.052
56	32 Orionis	5	21	5	48	N	March 7, September 9	.013
59	33 Orionis	5	22	3	9	N	March 6, September 8	.020
67	ζ Orionis	5	32	2	3	S	March 18, September 20	.025
366	41 Aurigæ	5	58	48	44	N	March 21, September 23	.077
69	8 Monocerotis	6	14	4	41	N	March 21, September 24	.136
76	38 Geminorum	6	44	13	24	N	April 1, October 4	.049
80	δ Geminorum	7	9	22	18	N	April 5, October 9	.064
88	11 Cancri	7	58	28	0	N	April 16, October 19	.041
93	φ^2 Cancri	8	16	27	31	N	April 19, October 23	.051
94	18 Bode, Hydræ	8	26	7	15	N	April 26, October 29	.096
96	144 of 145	8	39	71	27	N	April 20, October 23	.076
98	57 Cancri	8	43	31	16	N	April 27, October 30	.020
99	17 Hydræ	8	47	7	17	S	May 12, November 14	.053
102	Cancri 194	8	57	23	42	N	April 30, November 2	.067
114	Leonis 145	10	11	7	22	N	May 23, November 24	.060
128	90 Leonis	11	25	17	48	N	June 4, December 6	.039
133	65 Ursæ	11	46	47	29	N	May 21, November 23	.035
134	2 Comæ	11	55	22	28	N	May 17, November 18	.038
147	118 of 145	12	25	75	46	N	June 4, December 6	.053
152	Str. 422	12	40	4	48	N	June 30, December 30	.089
155	Str. 424	12	44	16	0	N	May 29, November 30	.072
161	54 Virginis	13	4	17	51	S	January 13, July 15	.060
167	81 Virginis	13	28	6	57	S	January 14, July 15	.038
73	98 of 145	14	5	6	14	N	January 15, July 17	.054

Specimen of a List of Stars—*continued.*

General Number in Messrs. H. and S's. Observations.	Star's Name, &c.	R. A. for 1820.	Declination for 1820.	Times of the year most proper for Observation.	Amount of Annual Parallax indicated by a periodical variation of 30′ in the angle of pos.
		h. m.	° ′		
176	Str. 456......	14 13	6 56 S	January 23, July 26...:	0″.061
177	Str. 457......	14 14	9 16 N	January 14, July 16....	.064
188	39 Bootis......	14 44	49 27 N	April 2, October 5.....	.040
193	44 Bootis.......	14 58	48 21 N	February 10, August 13	.020
194	Str. 474.......	14 59	9 55 N	February 2, August 4..	.042
201	η Coronæ B....	15 16	30 57 N	January 28, July 31....	.014
205	δ Serpentis?....	15 26	11 9 N	February 6, August 9..	.027
206	Libræ 178... .	15 30	8 11 S	February 11, August 14	.104
211	II. 85..........	15 47	1 39 S	February 7, August 10.	.085
212	III. 103........	15 48	3 56 N	January 31, August 2..	.132
228	g 5 Ophiuchi..	16 15	23 1 S	February 25, August 29	.036
240	—	16 46	19 15 S	March 3, September 5..	.064
245	39 Ophiuchi...	17 7	24 5 S	March 8, September 10.	.111
262	100 Herculis...	18 1	26 5 N	March 22, September 24	.125
265	I. 86..........	18 12	25 28 N	March 21, September 24	.040
269	39 Draconis...	18 21	58 42 N	April 28, November 1..	.031
271	—	18 30	41 7 N	March 22, September 25	.053
274	—	18 36	10 39 S	March 28, October 1...	.049
280	—	18 42	10 47 N	April 5, October 8.....	.042
287	—	18 58	6 53 N	April 2, October 5.....	.077
295	III. 57........	19 19	20 46 N	April 7, October 10....	.062
306	ϖ Aquilæ......	19 41	11 22 N	April 8, October 11....	.021
311	ε Draconis.....	19 49	69 48 N	April 20, October 23...	.023
312	ψ Cygni.......	19 51	51 38 N	January 8, July 9......	.038
313	I. 96..........	19 56	35 32 N	May 19, November 20..	.022
317	II. 96.........	20 3	0 19 N	April 30, November 2..	.045
320	I. 95..........	20 14	54 48 N	January 13, July 15....	.035
323	ϱ Capricorni II..	20 20	18 24 S	April 22, October 26 ..	.036
326	—	20 32	38 5 N	May 26, November 27 .	.085
343	Str. 751......	22 16	65 50 N	March 2, September 4..	.033
349	Aquarii 213....	22 34	9 11 S	May 28, November 28 .	.031
352	—	22 59	31 51 N	February 24, August 28	.076
354	94 Aquarii	23 10	14 26 S	May 20, November 21..	.133
356	107 Aquarii ...	23 37	19 41 N	June 3, December 5....	.045
359	σ Cassiop......	23 50	54 45 N	January 11, July 13....	.026

If I am not mistaken, then, we have here a method of investigating parallax very much more delicate than any which has yet been proposed—open to no insuperable objections—demanding no laborious reductions—and depending on a branch of practical astronomy, which is now happily cultivated with the assiduity it so richly deserves. It may not then be too much to hope, that in a few years we shall have data for a positive decision, respecting at least a certain number of individual stars, and thus be enabled to form a much better judgement as to the probable maximum of parallax in any. Meanwhile, the number of stars to which the method is applicable must not be judged of from the specimen above given. The only position micrometer of which I have any knowledge from practice is the wire micrometer; but the double image micrometer, if used in a manner, for the knowledge of which I am indebted to Captain KATER, and which consists in bringing the 1st image of one star almost, but not quite in contact with the 2nd image of the other, and in the same straight line, presents the singular advantage of an attainable exactness proportional to the distance of the stars; and by employing angles of position thus measured, stars of the 4th, 5th and 6th classes are rendered equally, or more available than those of the 1st, 2nd and 3rd; so that our range of objects becomes, by the use of this instrument in this manner really unlimited.

It is possible, that by some an apology may be thought necessary for a communication like the present—offered as it is to the Royal Society as a mere proposal—not followed out into actual practice. A variety of other pursuits, however, and circumstances less under my own controul, have hitherto prevented, and will in all probability continue to prevent, my engaging actively in the very extensive and laborious series of observations required for this purpose. Should I do so in future, to which however I by no means intend to pledge myself, my progress must of necessity be extremely slow. But should the method itself appear really to possess the advantages I am inclined to ascribe to it, practical astronomers I am sure will hardly impute its publication, even unaccompanied with observations, as a fault; and if their energy and perseverance should anticipate me in its application, I hope I should be the first to acknowledge that the merit of the discovery of parallax must rest with him, who, whatever be the method he may pursue, shall first point out the star in which it exists, and establish it to the satisfaction of astronomers by unequivocal observations.

J. F. W. H.

London, Dec. 8, 1825.

XX. *A formula for expressing the decrement of human life. In a letter addressed to Sir* Edward Hyde East, *Bart. M. P. F. R. S. By* Thomas Young, *M. D. For. Sec. R. S. Communicated February 2, 1826.*

Read April 19, 1826.

My Dear Sir,

The investigation of the laws, by which the general mortality of the human species appears to be governed, is of equal importance to the statesman, the physician, the natural philosopher, and the mathematician; and as you have had occasion to pay particular attention to the subject, I trust that it will not be disagreeable to you to receive the results of an inquiry, into which I have entered, for the purpose of appreciating, if not of reconciling, the many discordant opinions that have been advanced, respecting the comparative mortality of mankind, at different times, and under different circumstances.

Of late years, there is little doubt, that, whether from the protective effects of vaccination in infancy, or from the increase of the comforts of the poorer, and of the temperance of the more affluent classes of society, or in some measure also from the simplification of the practice of physic and surgery, there is a decided increase in the mean duration of life in many parts of Europe: but it is also extremely probable that this improvement has been greatly exaggerated; partly on account of the limited description of the persons on whom the observations have been made, and partly from an erroneous opinion respecting the profits of certain establishments, which have been attributed to the employment of too low an estimate of mortality, while they have, in fact, been principally derived from the high rate of interest which the state of public credit has afforded.

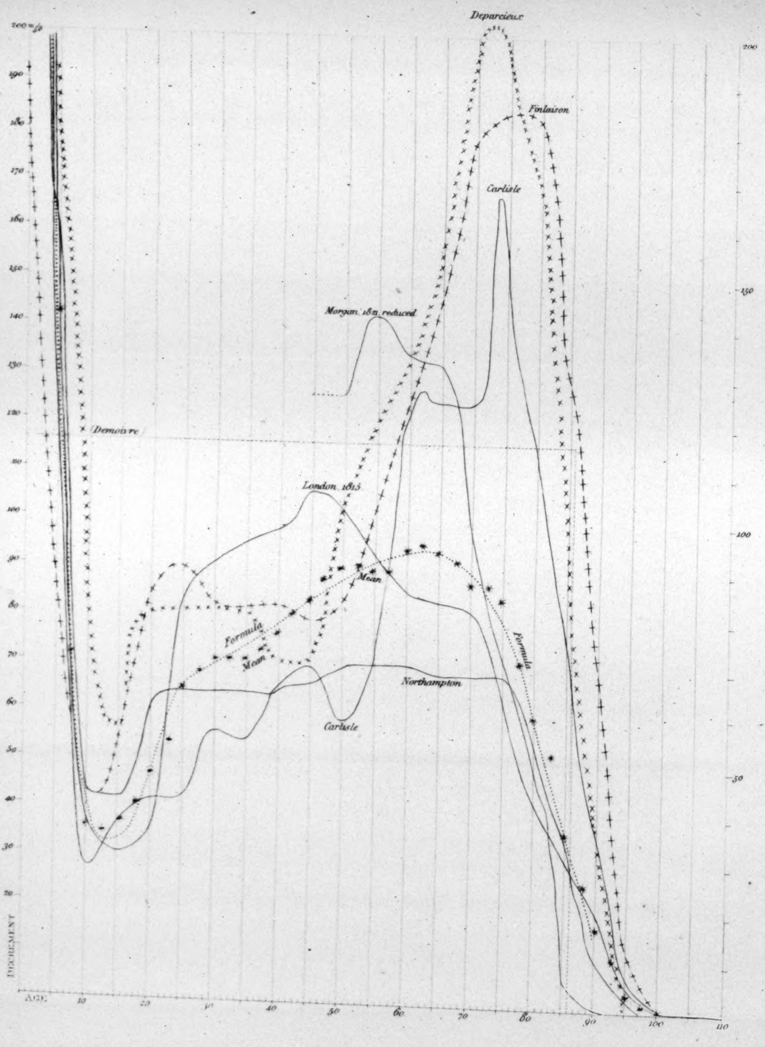
A very laborious and well informed actuary has lately asserted, before a Committee of the House of Commons, that "the duration of existence now, compared with what it was a hundred years ago, is as four to three, in round numbers." (Parl. Rep. N. 522, p. 44.) It does indeed happen, that this particular result may in one sense be very correctly deduced from the immediate comparison of the annual mortality of a certain number of persons of the same description, that is, annuitants, at the periods in question; nor is it possible to deny that some importance must be attached to the remark: but the mortality of the same class of persons in France, at the earlier period, was no greater, according to Mr. DEPARCIEUX's estimate of their longevity, than in England at the later, while the general mortality in France has never been materially less than in England, and appears at present to be even somewhat greater: and it can only be conjectured, that the annuitants of the tontine of King WILLIAM were in general most injudiciously selected, while those who were the subjects of Mr. DEPARCIEUX's observations, like the annuitants of the modern tontines, were chosen with more care, or with greater success. Mr. FINLAISON's tables, therefore, though they may be extremely just and valuable for the purpose of setting a price upon annuities to be granted on the lives of the proposers, cannot, with any prudence, be adopted where the parties concerned have an interest in offering the worst lives that they can find; notwithstanding any partial security that might be afforded by the exercise of medical skill in their rejection; and if it is true, that some of the tontines were principally filled by lot (Rep. p. 16), with the children of country clergymen and magistrates, it must still be supposed that the families of such persons may have been more healthy than the average of the population of London and the country taken together.

For the comparison of the general characters of different tables of mortality, the simplest and most obvious criterion is perhaps the number of individuals out of which one dies annually, which is also the number of years expressing the expectation of life at the time of birth. But this test is liable to material objections with regard to the most usual application of the table, which depends more on the comparative expectations at later periods than in early infancy. For example; the Northampton table affords results, throughout the whole of middle and advanced life, agreeing almost exactly with DEMOIVRE's hypothesis of equal decrements, although the *annual mortality* is supposed to be nearly 1 in 25 at Northampton, instead of 1 in 43, as assumed by DEMOIVRE. It would therefore be very unjust for a person allowing the truth of DEMOIVRE's hypothesis, to condemn the practical employment of Dr. PRICE's tables in common cases, on account of this variation only. A less exceptionable test will be, to find the mean of the numbers expressing, for different ages, the *full term* of life, or the sum of the age and twice the expectation, taking the decads from 10 to 80 as the most important. Another standard of comparison may be the age which is equal to the expectation of life, and which, in Demoivre's arithmetical hypothesis, is the *mean age* of all the population, and probably very near it in all tables formed from actual observation. In this manner a general comparison of the most remarkable tables may be instituted.

Characteristics of Mortality.

	Annual mortality one in	Mean full term of life.	Mean age.
Roman estimate of Ulpian, probably with some deduction for present value.........	.....	74 (+ disc^{t})	26 (+ disc^{t})
Deparcieux's Tontines, beginning 1689	47.67	94.17	32.5
Halley's table for Breslau, 1690............	33.50	87.15	28.1
Tontine of 1695, Finlaison, males..........	37.61	83.42	27.25
——————— females	(43.0)	87.50	29.32
Simpson's table for London, about 1730	19.2	82.30	25.7
Dupré, in Buffon, about 1750	33.0	85.30	28.67
Northampton tables, about 1760...........	25.18	87.39	28.86
Swedish tables, about 1785................	36.12	91.86	31.3
France, before the Revolution, Duvillard	28.76	86.96	29.0
Finlaison's tontine and annuitants, about 1800, males..............................	50.16	93.25	32.0
females	55.51	100.7	34.6
Finlaison's Chelsea Pensioners	.....	90.0	29.65
Carlisle tables, about 1810, Milne..........	37.14	95.47	32.6
Returns for all England, 1811	49		

The order of the mortalities expressed by the first column of this table, is, Simpson, Northampton, France, Dupré, Halley, Sweden, Carlisle, tontine 1695 males, females, Deparcieux, returns of 1811, tontines of 1800, males, and females; the order of the second column is Simpson, tontine of 1695, males, Dupré, France, Halley, Northampton, females 1695, pensioners, Sweden, tontines of 1800, males, Carlisle, females of 1800: but besides this difference in the order, the disproportion exhibited in this column is less enormous than in the former; the numbers of the Carlisle


200
190
180
170
160
150
140
130
120
110
100
90
80
70
60
50
40
30
20
DECREMENT
AGE
10
20
30
40
50
60
70
80
90
100
110
200
150
100
50
Deparcieux
Finlaison
Carlisle
Morgan 1821 reduced
(Demoivre)
London 1815
Mean
Formula
Formula
Mean
Northampton
Carlisle

J. Basire sc.

tables, for example, exceeding those of the Northampton by one half in the former, and by one tenth only in the latter. The proportion of Mr. FINLAISON's tontines also stands as 3 to 4 in the first, and as 7 to 8, or 8 to 9 only in the second: the latter comparison giving a much fairer practical estimate of the comparative longevity, indicated by the tables, than the former.

Another mode of easily appreciating the regularity and the analogies of different tables is, to construct a diagram, in the form of a curve, of which the absciss represents the age, and the ordinates the corresponding decrements of life. (Plate XI.) The inspection of such a diagram is sufficient to convince us of the great irregularity of the Carlisle tables of mortality, which must obviously have been formed, as they confessedly were, from observations on a very limited number of individuals, so that they exhibit a succession of different climacterics, after which the mortality is diminished, while about the age of 74 the curve that represents them towers to an incredible height, affording an expectation of longevity which some of the strongest advocates of those tables have abandoned in their practical applications, since they take their estimate of life, in advanced age, even lower than it is represented in the Northampton tables.

It appears therefore to be highly probable, that the fairest basis for general computations, to be applied throughout Great Britain, may be obtained by a proper combination of the tables of Northampton, which have been long known and very generally approved, with the Carlisle tables, corrected however in their extravagant values of old lives, by some other documents; and with the mortality of London as derived from the parish registers, which, when thus incorporated with tables formed in the country, will be freed from the objections that have been made to the observations of burials in great cities only.

The Carlisle table agrees in the earlier parts pretty nearly with the observations of Mr. MORGAN on the experience of the Equitable Office from 1768 to 1810, as it appears from Mr. MILNE's comparison, as well as from the reduction and interpolation of those observations published by Mr. GOMPERTZ in the Philosophical Transactions for 1825: but for correcting the later portions of the Carlisle table, it may be allowable to employ a subsequent register of the experience of the Equitable Office, so far as it is possible to make any inferences from it with safety.

The numbers of deaths occurring in 20 years, as recorded by Mr. MORGAN, might have been made the foundation of a very valuable determination of the mortality occurring in a certain class of persons, if the number of the Equitable Society had become stationary before the commencement of the record: but in order to deduce from it a just estimate of the value of life, it would then be necessary to alter the numbers of deaths at each age, in the inverse proportion of the numbers of the living compared: that is to say, not simply of the sums of the persons admitted under that age, but of the numbers of persons born whom they represent: since, in comparing the joint mortalities of any two lists of persons, we must obviously add together the deaths belonging, not to a given number of persons of various ages, but of a number proportionate to the survivors at the respective ages out of a given number of births, so that in this manner the apparent mortality in the earlier portions of the register would require to be augmented, not only on account of the smaller number of persons who have actually contributed to furnish it, but also on account of the greater proportion that these persons bear to the corresponding number at birth, when compared with the survivors at more advanced ages, who represent a population still more exceeding their own numbers. On the other hand, since the register in question relates only to a limited number of years, immediately following a very rapid increase of the Society, it is evident that the deaths must have occurred at earlier ages than if it had been continued until all the lives had dropped.

Of these three modifications, it may be sufficiently accurate for the present purpose to omit the two latter as nearly counterbalancing each other, and to augment the earlier numbers in the proportion only of the members of the Society to whom they must necessarily have belonged, supposing that the admissions had taken place about the same ages at all periods; assuming also the number of survivors at 45 to be in the same proportion to the births as in the Carlisle table. We may then proceed to take a mean between the mortality thus obtained, with proper interpolations, and the observations at Carlisle, as the second of the three principal bases to be afterwards incorporated with the mortality of Northampton and of London. Further than this, it is impossible to place any great reliance on Mr. MORGAN's document, which makes the annual deaths, in "a population exceeding 150000," not quite 1 in 1500.

Of the mortality of London, taken for the ten years from 1811 to 1820, it may be observed, that its results bear the internal evidence of greater apparent correctness than either of the other bases, exhibiting a curve less irregular in its flexures, and generally intermediate between the others: it has also the advantage of exhibiting the duration of life as prolonged by the general introduction of vaccination: and when thus incorporated with the registers of two places in the country, each reduced to an equal supposed population, it must probably be sufficiently corrected for the errors that may be attributed to the effect of an afflux of settlers at an early age. The mean obtained in this manner might be employed at once as a standard table without much inconvenience, but it still exhibits some minute but obvious irregularities, as an inspection of the line of stars in the diagram will show, principally perhaps from the want of skill or care with which the interpolations have been made by Dr. PRICE and others. The most effectual of all interpolations for *harmonizing* the various orders of differences, is to obtain a formula which shall extend with sufficient accuracy throughout the whole curve. It may be easily believed that it must be extremely difficult to find such an expression; and that its form must be too complicated to be applied to any practical purpose throughout its extent. I have however drawn a curve which comes extremely near to the line of stars, and crosses it in 10 or 12 different points, by means of the equation $y = 368 + 10x - 11.(156 + 20x - xx)^{\frac{3}{2}} + \frac{1}{285 + 2.05xx + 2\left(\frac{x}{10}\right)^6} - 5.5\left(\frac{x}{50}\right)^{10} + \frac{5.5^2}{4000}\left(\frac{x}{50}\right)^{20} - 5500\left(\frac{x}{100}\right)^{40}$: y being the number of deaths among 100000 persons, in the year that completes the age x.

The terms of this formula have some remarkable relations to the different periods of life. HALLEY's first approximation was $y = 1000$, throughout life. DEMOIVRE's arithmetical hypothesis was $y = \frac{100000}{86} = 1163$: but of the present formula the principal foundation, as extending to the whole of life, is, $y = 368 + 10x$. In infancy the term containing the reciprocal of the powers of x has a preponderating value: in youth, the term $-(156 + 20x - xx)^{\frac{1}{2}}$, which diminishes the mortality, ends somewhat abruptly at 25, and would be incapable of being employed with safety in algebraical calculations, from its having a negative as well as a positive value. Old age is expressed almost exclusively by the high powers at the end of the formula, which terminate the series with great and increasing rapidity. It is obvious that for many purposes of calculation, the terms belonging to youth and to old age might be neglected without inconvenience, and that, for the middle portion of life, the terms $368 + 10x$ alone, with some little modification, might be employed as sufficiently correct; or certainly as much nearer to the truth than either the arithmetical or geometrical hypothesis of DEMOIVRE. The relations of the different parts of the formula will be best appreciated from their developement in the following tables.

DOCUMENTS.

Deaths among persons assured by the Equitable Society, for 20 *years, from* 1800 *to* 1821.—Morgan on Assurances, 1821, p. 325.

Age.	Deaths.	Assured.	Deaths cor.	Interpolations.			
10 to 20	7	1494		251	514	631	630
20 to 30	37	8996		275	566	636	630
30 to 40	166	33850		299	606	646	630
40 to 50	299	45429	630	323	616	655	630
50 to 60	458	36489	649	347	618	663	630
60 to 70	536	19042	604	371	620	665	
70 to 80	345	6454	359	395	622	663	
above 80	82		82	419	624	657	
				440	626	644	
		151754		470	628	630	(820)
				3590	6040	6490	20090
						Carlisle	45070

Mortality of London from 1811 *to* 1820.—Gent. Mag.

Between 0 & 2		2 & 5	5&10	10&20	20&30	30&40	40&50	50&60	60&70	70&80	80&90	90&100	100	101	102	103	104	105	108	109	111	113
1811	5106	1638	654	509	1231	1641	1741	1591	1385	1038	449	56	1	1	1	1						
1812	5636	1907	655	620	1226	1685	1841	1543	1425	1193	492	71	0	0	1							
1813	5167	1733	604	526	1108	1501	1751	1606	1559	1211	489	61	1	1	1	0	0	0	0	2	0	1
1814	5845	2038	770	649	1268	1678	1950	1810	1747	1343	592	88	1	1	1	0	0	0	1	0	1	
1815	5200	1916	870	677	1425	1824	2075	1886	1621	1221	674	167	2	1	0	1						
1816	5400	1960	845	675	1464	1912	2123	1955	1720	1308	781	168	3	0	0	1	1					
1817	5698	2019	929	706	1364	1795	1983	1788	1614	1224	683	156	7	0	0	0	0	2				
1818	5381	1815	808	703	1453	1884	2040	1864	1585	1271	722	175	1	1	1	0	0	0	1			
1819	4779	1771	826	631	1577	1990	2095	1918	1600	1230	666	144	0	0	0	1						
1820	4758	1975	887	667	1484	2006	2069	1878	1632	1208	662	119										
	52970	18772	7848	6363	13600	17916	19668	17839	15888	12247	6210	1205	16	5	5	4	1	2	2	2	1	1
													(39)			190565						

Interpolations.

Age.	Deaths.	Age.	Deaths.	Age.	Deaths.	Age.	Deaths.	Age.	Deaths.	Age.	Deaths.
0	32970	20	710	40	1855	60	1622	80	529	100	16
1	20000	21	770	41	1885	61	1615	81	768	101	5
	52970										
2	8500	22	960	42	1925	62	1607	82	718	102	5
3	6000	23	1160	43	1963	63	1600	83	678	103	4
4	4272	24	1460	44	1990	64	1592	84	638	104	2
	18772										
5	2800	25	1680	45	2010	65	1585	85	597	105	2
6	1800	26	1700	46	2020	66	1577	86	556	106	1
7	1400	27	1710	47	2020	67	1570	87	516	107	1
8	1008	28	1720	48	2010	68	1563	88	475	108	1
9	840	29	1730	49	1990	69	1557	89	435	109	1
	7848		13600		19668		15888		6210		38
10	740	30	1741	50	1959	70	1550	90	380	110	1
11	660	31	1752	51	1920	71	1478	91	290	111	0
12	615	32	1763	52	1880	72	1405	92	180	112	0
13	605	33	1774	53	1840	73	1333	93	90	113	0
											1
14	600	34	1786	54	1800	74	1261	94	70		
15	603	35	1798	55	1760	75	1188	95	57		
16	610	36	1809	56	1720	76	1116	96	48		
17	620	37	1820	57	1680	77	1044	97	40		
18	640	38	1831	58	1650	78	972	98	30		
19	670	39	1842	59	1630	79	900	99	20		
	6363		17916		17839		12247		1205		

Comparative Decrements from various Tables.

Age.	North-ampton.	Carlisle.	Equitable Office Red.	Mean of Carlisle and Eq. Office.	London Bills.	General Mean.	Living.
0	25751	15390			17301	19481	99124
1	11734	6820			10493	9682	79643
2	4309	5050	—	—	4460	4606	69961
3	2876	2760			3148	2928	65355
4	1691	2010			2242	1981	62327
5	1579	1210			1469	1419	60346
6	1202	820			945	989	58927
7	944	580	—	—	725	750	57938
8	687	430			529	549	57188
9	515	330			441	429	56549
10	446	290			389	375	56120
11	429	310			346	362	55745
12	429	320	—	—	323	357	55383
13	429	330			318	359	55026
14	429	350			315	365	54667

Comparative Decrements from various Tables.

Age.	Northampton.	Carlisle.	Equitable Office Red.	Mean of Carlisle and Equi. Office.	London Bills.	General Mean.	Living.
15	429	390			317	379	54302
16	455	420			320	398	53923
17	497	430	—	—	325	417	53525
18	541	430			335	435	53108
19	575	430			352	452	52673
20	618	430			372	473	52221
21	644	420			404	489	51748
22	644	420	—	—	503	522	51259
23	644	420			608	557	50737
24	644	420			766	610	50180
25	644	430			882	652	49570
26	644	430			892	655	48918
27	644	450	—	—	897	664	48263
28	644	500			902	682	47599
29	644	560			907	704	46917
30	644	570			913	709	46213
31	644	570			919	711	45504
32	644	560	—	—	925	710	44793
33	644	550			931	708	44083
34	644	550			937	710	43375
35	644	550			943	712	42665
36	644	560			950	718	41953
37	644	570	—	—	955	723	41235
38	644	580			961	728	40512
39	644	610			967	740	39784
40	652	660			974	762	39044
41	661	690			990	780	38282
42	669	710	—	—	1010	796	37502
43	669	710			1030	803	36706
44	669	710			1044	808	35903
45	669	700	1346	(765)	1055	830	35095
46	669	690	1346	(821)	1059	850	34265
47	669	670	1346	(873)	1059	867	33415
48	669	630	1346	(916)	1055	880	32548
49	678	610	1346	978	1044	900	31668
50	695	590	1346	968	1028	897	30768
51	704	620	1375	997	1007	903	29871
52	704	650	1404	1027	987	906	28968
53	704	680	1416	1048	966	906	28062
54	704	700	1421	1060	945	903	27156
55	704	730	1416	1073	924	900	26253
56	704	760	1399	……	902	894	25353
57	704	820	1381	……	881	895	24459
58	704	930	1359	……	866	904	23564
59	704	1060	1348	……	856	921	22660

Comparative Decrements from various Tables.

Age.	North-ampton.	Carlisle.	Equitable Office Red.	Mean of Carlisle and Equit. Office.	London Bills.	General Mean.	Living.
60	704	1220	1342	......	851	945	21739
61	704	1260	1338	......	848	950	20794
62	695	1270	1333	......	844	948	19844
63	695	1250	1329	......	840	941	18996
64	687	1250	1325	......	836	936	18055
65	687	1240	1321	......	832	936	17119
66	687	1230	1317	......	828	929	16183
67	687	1230	1295	......	824	925	15254
68	687	1230	1210	......	819	909	14329
69	687	1240	1098	......	817	889	13420
70	687	1240	1005	......	813	874	12531
71	687	1340	940	......	776	867	11657
72	687	1460	895	......	738	868	10790
73	687	1560	844	......	700	863	9922
74	687	1660	792	......	661	858	9059
75	687	1600	742	......	623	839	8201
76	661	1560	690	......	585	790	7362
77	627	1460	639	......	548	741	6572
78	584	1320	587	......	510	683	5831
79	548	1280	536	......	472	643	5148
80	541	1160	480	......	435	599	4505
81	515	1120	440	......	402	549	3906
82	489	1020	300	......	376	508	3357
83	472	940	200	......	356	466	2849
84	412	840	100	......	335	406	2383
85	351	780	60	......	313	361	1977
86	291	710	40	......	292	319	1616
87	241	640	30	......	271	284	1297
88	180	510	20	......	251	234	1013
89	137	390	10	......	230	190	779
90	103	370	8	......	199	164	589
91	86	300	7	......	152	130	425
92	69	210	7	......	94	87	295
93	60	140	6	......	47	60	208
94	43	100	6	......	37	44	148
95	26	70	5	......	30	31	104
96	9	50	5	......	25	19	73
97	0	40	5	......	21	14	54
98	..	30	4	......	16	9	40
99	..	20	4	......	10	6	31

Comparative Decrements from various Tables.

Age.	North-ampton.	Carlisle.	Equitable Office Red.	Mean of Carlisle and Eq. Office.	London Bills.	General Mean.	Living.
100	..	20	4	......	8	6	25
101	..	20	3	......	3	5	19
102	..	20	3	......	3	5	14
103	..	20	2	......	2	4	9
104	..	10	2	......	[1]	2	5
105	..	0	1	......	1	1	3
106	..	..	0	......	.5	(.25	(2.00
107	..	..	....	......	.5	.25	1.75
108	..	..	....	......	.5	.25	1.50
109	..	..	....	......	.5	.25	1.25
110	..	..	....	......	.5	.25	1.00
111	..	..	....	......	...	.25	.75
112	..	..	....	......	...	.25)	.50
113	..	..	....	......	...	.25)	.25)
114	..	..	....	......	...	.0	.00)

Decrements of Mortality computed from the Formula.

Age $(x-1)$	$368 + 10x$	$-.11(156+20x - xx)^{\frac{3}{2}}$	$+\frac{1}{2.85+2.05xx+2\left(\frac{x}{10}\right)^6}$	Decrement.
0	378	−255	+ 20408	20531
1	388	241	9009	9106
2	398	313	4695	4780
3	408	359	2805	2854
4	418	386	1848	1880
5	428	409	1322	1341
6	438	427	968	979
7	448	440	746	752
8	458	447	592	603
9	468	451	477	494
10	478	447	392	423
11	488	440	329	377
12	498	427	278	349
13	508	409	238	337
14	518	386	205	337

Decrements of Mortality computed from the Formula.

Age $(x-1)$	$368 + 10x$	$-.11(156+20x-xx)^{\frac{3}{2}}$	$+\frac{1}{2.85+2.05xx+2\left(\frac{x}{10}\right)^6}$	$-5.5\left(\frac{x}{50}\right)^{10}$	Decrement.
15	528	359	178	......	347
16	538	313	156	......	381
17	548	291	136	......	393
18	558	255	119	......	422
19	568	214	104	......	458
20	578	174	93	......	497
21	588	130	82	......	540
22	598	89	72	......	581
23	608	51	64	......	621
24	618	19	57	......	656
25	628	0	50	......	678
26	638	......	44	......	682
27	648	......	39	......	687
28	658	......	34	......	692
29	668	......	30	......	698
30	678	......	27	......	705
31	688	......	24	......	712
32	698	......	21	......	719
33	708	......	18	......	726
34	718	......	16	......	734
35	728	......	14	......	742
36	738	......	13	......	751
37	748	......	11	—0	759
38	758	......	10	.4	768
39	768	......	9	.6	776
40	778	......	8	.7	785
41	788	......	8	.9	795
42	798	......	7	1.2	804
43	808	......	6	1.5	813
44	818	......	5	1.9	821
45	828	......	5	2.3	831
46	838	......	4	3.0	839
47	848	......	4	3.9	848
48	858	......	3	4.5	857
49	868	......	3	5.5	866
50	878	......	3	6.7	874
51	888	......	2	8	882
52	898	......	2	10	890
53	908	......	2	12	898
54	918	......	2	14	906

Decrements of Mortality computed from the Formula.

Age $(x-1)$	$368 + 10x$	$-*+\frac{1}{2.85+2.05xx+2\left(\frac{x}{10}\right)^6}$	$-5.5\left(\frac{x}{50}\right)^{10}$	$+.001\left(\frac{5.5\left(\frac{x}{50}\right)^{10}}{2}\right)^2$	$-5500\left(\frac{x}{100}\right)^{40}$ =	Decrement.
55	928	2	17	....	....	913
56	938	1	20	....	....	917
57	948	1	24	....	....	923
58	958	1	28	....	....	929
59	968	1	33	....	....	934
60	978	1	39	+0	....	938
61	988	1	46	1	....	942
62	998	1	55	1	....	943
63	1008	1	64	1	....	944
64	1018	1	75	1	....	943
65	1028	......	−88	2	....	942
66	1038	......	102	3	....	939
67	1048	......	119	4	....	933
68	1058	......	137	5	....	926
69	1068	......	159	6	....	915
70	1078	......	183	8	....	903
71	1088	......	211	11	....	888
72	1098	......	242	15	....	871
73	1108	......	277	19	....	850
74	1118	......	317	25	....	826
75	1128	......	359	32	....	801
76	1138	......	412	42	....	768
77	1148	......	470	55	....	733
78	1158	......	532	71	−0	697
79	1168	......	604	91	1	654
80	1178	......	684	117	1	610
81	1188	......	772	145	2	559
82	1198	......	872	190	3	513
83	1208	......	984	242	6	460
84	1218	......	1108	307	9	408
85	1228	......	1243	386	14	357
86	1238	......	1399	490	22	307
87	1248	......	1567	614	37	258
88	1258	......	1756	771	52	215
89	1268	......	1963	963	86	178
90	1278	......	2192	1201	139	148
91	1288	......	2444	1493	212	125
92	1298	......	2713	1849	333	101
93	1308	......	3032	2300	496	80
94	1318	......	3371	2840	734	53
95	1328	......	3744	3504	1041	27
(96	1338	......	4150	4306	1746	0
97	1348					
98	1358					
99	1368					
100)	1378					

MEAN STANDARD TABLE OF THE DECREMENTS OF LIFE IN GREAT BRITAIN, 1824.

Age.	Decrement.	Living.	Age.	Decrement.	Living.	Age.	Decrement.	Living.	Age.	Decrement.	Living.
0	20531	100003	30	705	46527	60	938	21810	90	164	589
1	9106	79472	31	712	45822	61	942	20872	91	130	425
2	4780	70366	32	719	45110	62	943	19930	92	87	295
3	2854	65586	33	726	44391	63	944	18987	93	60	208
4	1880	62732	34	734	43665	64	943	18043	94	44	148
5	1341	60852	35	742	42931	65	942	17100	95	31	104
6	979	59511	36	751	42189	66	939	16158	96	19	73
7	752	58532	37	759	41438	67	933	15219	97	14	54
8	603	57780	38	768	40679	68	926	14286	98	9	40
9	494	57177	39	776	39911	69	915	13360	99	6	31
10	423	56683	40	785	39135	70	903	12445	100	6	25
11	377	56260	41	795	38350	71	888	11542	101	5	19
12	349	55883	42	804	37555	72	871	10654	102	5	14
13	337	55534	43	813	36751	73	850	9783	103	4	9
14	337	55197	44	821	35938	74	826	8933	104	2	5
15	347	54860	45	831	35117	75	801	8107	105	1	3
16	381	54513	46	839	34286	76	768	7306	106	.25	2
17	393	54132	47	848	33447	77	733	6538	107	.25	1.75
18	422	53739	48	857	32599	78	697	5805	108	.25	1.50
19	458	53317	49	866	31742	79	654	5108	109	.25	1.25
20	497	52859	50	874	30876	80	610	4454	110	.25	1.0
21	540	52362	51	882	30002	81	559	3844	111	.25	.75
22	581	51822	52	890	29120	82	513	3285	112	.25	.50
23	621	51241	53	898	28230	83	460	2772	113	,25	.25
24	656	50620	54	906	27332	84	408	2312	114	0	0
25	678	49964	55	913	26426	85	357	1904			
26	682	49286	56	917	25513	86	307	1547			
27	687	48604	57	923	24596	87	258	1240			
28	692	47917	58	929	23673	88	215	982			
29	698	47225	59	934	22744	89	178	767			

I shall take this opportunity of endeavouring to demonstrate, in a simple and undeniable manner, the error into which Dr. PRICE and his followers have fallen, in consequence, as it appears, of their adopting the legal restraints on usury as essential steps in the mathematical calculation of the amount of compound interest. The error has indeed of late years been very commonly admitted; but its effects have not been so satisfactorily rectified as could be desired.

In the 66th volume of the Philosophical Transactions, for the year 1776, we find a Paper of Dr. Price, in which he lays down these theorems, r denoting the interest of £1. for a year, and n the term or number of years during which any annuity will be paid, p the perpetuity, or $\frac{1}{r}$, y the value of an annuity paid yearly, and h half yearly: then, I, $y=p-\frac{1}{r(1+r)^n}$; and, II, $h=p-\frac{1}{r\left(1+\frac{r}{2}\right)^{2n}}$: and as examples, taking $r=.04$, and $n=5$, we have $y=4.4518$, and $p=4.4913$.

Now, if we analyse the results thus obtained, by dividing them into the present values of the separate payments, they will stand thus;

I. Present value of £1. payable at the end of 1 year.	- £.	.961538
2 years	- -	.924556
3 years	- -	.888996
4 years	- -	.854804
5 years	- -	.821927
		4.451821

II. Present value of 10 shillings, payable at the end of half a year	£.	.49020
1 year	-	.48058
1½	- - -	.47127
2	- - -	.46192
2½	- -	.45286
3	- - -	.44398
3½	- -	.43528
4	- - -	.42674
4½	- - -	.41837
5	- - -	.41018
		4.49138

The present values of 10 shillings are therefore assumed;

I, at 1 year .48077 ; II, .48058
2 years .46228 .46192
3 years .44450 .44398
4 years .42740 .42674
5 years .41096 .41018

The latter column exhibiting obviously a larger deduction for discount than the former; so that the rate of interest in the two calculations is by no means the same: although in the case of $r = .05$, they would respectively represent the highest rate of interest allowed by our laws to be received without a new investment or engagement: but this arbitrary restraint ought certainly not to affect the mathematical consideration of the question.

The difficulty, if any person thinks it such, may be avoided by a mode of investigation which I have lately had occasion to point out. " An annuity, of which a payment is due on a given day, is more valuable than an annuity purchased on that day, and to commence a year after, by the amount of a year's payment: and *the value of a life annuity, becoming payable at any intermediate time between the day of purchase and its first anniversary, will be greater than the simple tabular value of the annuity by a sum proportional to the anticipation of the payment*;" the increase of the value being very nearly uniform, when we suppose the anticipation to be gradually increased: this increase of the value comprehending obviously the greater probability as well as the greater proximity of each payment, and proceeding from day to day by very nearly equal increments. Thus, if we wished to purchase an annuity of £100. a year, and its value were £1000., upon the ordinary supposition of the payments commencing after the end of a year; supposing that we desired to have the first payment made at the end of nine months, and the subsequent payments at annual intervals as usual, we should have to add £25. to the purchase money, making it £1025. at whatever rate of interest the value might have been computed. If we began at six months, £50., and if at three months, £75. must be added to the purchase: it being obvious that an additional £100. would be equivalent to an anticipation of twelve months, or to an immediate payment of a year's annuity.

From this simple and incontestable principle, in which the second differences only are neglected, it is very easy to deduce the values of annuities, payable at intervals shorter than a year. An annuity of 1, payable half yearly, is equal to two annuities of $\frac{1}{2}$, the one beginning as usual at the end of the year, the other anticipated by half a year; and the value of this portion is greater than the other by half of one of the payments, that is, by $\frac{1}{4}$: so that "*We may always find the value of a life annuity payable half yearly, by adding a quarter of a year to the tabular value of the same annuity.*"

In a similar manner it is very easily shown, that "*for quarterly payments, we must add $\frac{3}{8}$ of a year's value to the computation made on the supposition of annual payments;*" and "the continual bisection of the interval would at last afford us *the addition of half a yearly payment for the value of a daily or hourly payment of a proportional part of the given annuity.*"

"It may also be observed, that when we reckon at 3 per cent. interest, an annuity payable half yearly is the same, throughout the middle of life, that would be granted on the life of a person a year older, if payable annually."

If it is required to ascertain the value of a reversionary annuity payable half yearly or quarterly, the calculation becomes in appearance a little paradoxical; for since the true value of a reversionary annuity for the life of one person, for example, after the death of another, is the difference between the values of two annuities on the single life and the joint lives, and since an equal addition must be made to these values in consideration of the period of payment being shortened, it follows that the reversionary annuity must be of equal value in either form. This conclusion would indeed be strictly true if the periodical times of payment remained unaltered, according to the supposition from which the value of the annuities is deduced; while in fact it is usual to grant such an annuity to commence at the first quarterly, half yearly, or annual period after the contingent event: a variation which would have no sensible effect in the case of daily payments, but which lessens the value of reversionary annuities at other periods by that of half a payment for the given period, reduced to the present time in the same manner as any other sum assured as payable upon the same contingency of survivorship.

The simplicity observable in the progression of the values of annuities, calculated according to the values of lives here supposed, and at 3 per cent. interest, leads us to inquire what would be the exact law of mortality required to make that progression strictly uniform throughout life; and it will appear on investigation, that in order to have the value $24.45 - \frac{1}{4}x$,

x being the age of the person, which is nearly true between 20 and 70, the annual mortality must be expressed by $\frac{.03x + .066}{100.8 - x}$: a fraction which at 20 becomes $\frac{1}{121}$, at 40, $\frac{1}{48}$, at 60, $\frac{1}{22}$, and at 80, $\frac{1}{8.4}$. Our table gives respectively $\frac{1}{103}$, $\frac{1}{50}$; $\frac{1}{23}$, and $\frac{1}{7.3}$: the Northampton $\frac{1}{71}$, $\frac{1}{48}$, $\frac{1}{25}$, and $\frac{1}{7.4}$. Mr. Finlaison's male annuitants $\frac{1}{87}$, $\frac{1}{73}$, $\frac{1}{32}$, and $\frac{1}{8.3}$. The healthiness of Mr. Finlaison's annuitants about 40 and 50 is one of the most remarkable features of his table: he observes (p. 58), that out of 10,000 persons at 23, 141 will die in a year, and 141 will die out of the same number at the age of 48; but at the age of 34 there will only die 124. The curve marked by obelisks, +, in the diagram, will show the comparative progress of mortality in this system; which, however valuable the data may be, appears to exhibit too many novelties, if not anomalies, to be generally adopted with confidence: while the line of crosses, ×, representing the tontine of Deparcieux, will serve to show how little difference the lapse of a century has made in the results of these two similar cases.

I shall conclude, my dear Sir, with a comparison of the climacteric years, as they may be called without impropriety, in which the greatest numbers of adults die, as taken from different tables.

I sincerely hope that these considerations may help to undeceive the too credulous public, who have of late not only received some hints that tend to insinuate the probability of an occasional recurrence of a patriarchal longevity, but who have been required to believe, upon the authority of a most respectable mathematician, that the true and unerring value of life is not to be obtained by taking an average of various decrements, but by adopting the extreme of all conceivable estimates, founded only on a hasty assertion of Mr. MORGAN, and unsupported by any detailed report; an estimate which makes the grand climacteric of mankind in this country, not a paltry fifty four, or the too much dreaded sixty three; but no less than EIGHTY TWO! An age to which nearly one sixth of the survivors at ten are supposed to attain!

Climacterics, or greatest Decrements.

Berlin, formerly	38	Breslau, 1695	61	Montpellier, 1782	67	Deparcieux	73
London, about 1733	40	Formula	63	Duvillard, France	67	Carlisle	74
Paris, formerly	40	Brandenburg	65	Sweden, 1762	68	Ackworth, 1752	75
Stockholm, 1762	42	Warrington, 1777	65	Chester, 1776	68	Kersseboom	77
London, 1764	43	Norwich, 1765	66	Sweden, 1785	69	Finlaison	78
London, 1815	46			Holycross, 1760	70	E. O. Mr. B.	82
Northampton, 1757	56						

Believe me, dear Sir,

Your faithful and obedient Servant,

THOMAS YOUNG.

Park Square, 28 Feb. 1826.

XXI. *Account of an experiment on the elasticity of ice.* By BENJAMIN BEVAN, *Esq. In a Letter to Dr.* THOMAS YOUNG, *For. Sec. R. S.*

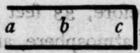
Read April 27, 1826.

DEAR SIR, **Norwich, January 18, 1826.**

I HAVE been long desirous of repeating my experiment on the elasticity of ice, but until the present frost have not had an opportunity for two years, when I had but a single specimen, and rather too small, and which broke soon after the commencement of the experiment. From that experiment I had calculated a modulus of 6.000,000 feet; but finding the result of my experiments in the present season much less, and pretty uniform amongst themselves, I re-examined the calculations made in 1824, and have discovered an error, which had before escaped me, in the reduction, and which brings the result of that experiment to agree with those made in the present season, or about 2.100,000 feet. I will not answer for the absolute correctness of the second figure, because a variation of the $\frac{1}{100}$ of an inch in the thickness of the specimen will change this figure.

The present severe frost has enabled me to try experiments on a much larger scale, and upon ice from $1\frac{1}{4}$ to 4 inches in thickness: to explain the mode I adopted with ice of near 4 inches in thickness may be necessary, and was as follows:

Upon a large pond within less than a mile of my residence at Leighton Bussard, the depth of which I found to be about

4 feet, I had a channel sawed on three sides of a parallelogram, in the following form, 

, separating the specimen to be operated upon, except at the end lettered *a*, where its union was left undisturbed. The dimensions of this prism was 100 inches long, 10 inches wide, and thickness at $a = 3.62$, at $b = 4.00$, and at $c = 3.75$ inches. To save some trouble in the calculation, I considered the thickness uniformly 3.97 inches or the mean of the whole: although I am aware that it would not be strictly accurate; but as the experiment was upon a large scale, and with weights up to 25 lbs. and with deflections proportional to the weights applied, I considered the experiment a fair one. In this experiment the weights were placed at 98 inches from the line of union with the main body, and the deflection by 25 lbs. was 0.206 inches; from which I estimate the modulus to be 2.100,000 feet.

After this, I repeated the experiment on ice of various dimensions and of different thickness, and in all, the result agreed quite as near as the admeasurement of the thickness could be ascertained, as well when the deflection was tried upon the water, as when the ice was taken out and tried in the manner used with wood and metal.

Now the modulus of water as given in your valuable lectures, derived from Canton's experiment, is, if I recollect right, about 750,000 feet, calculated from a condensation of $\frac{1}{22000}$ by the pressure of one atmosphere. This condensation, I presume, was *cubical* or solid; whereas the condensation used in regard to the modulus of elasticity is *linear*. You are of course aware that a linear compression of $\frac{1}{66000}$ will produce a cubical condensation of $\frac{1}{22000}$, as found by CANTON. Allowing, therefore, 33 feet for a column of water = the pressure of the atmosphere, and multiply this by 66000, we shall have 2.178,000 feet for the modulus of water, agreeing pretty nearly with the result of my experiments.

I should not have troubled you with this letter, but as the frost may yet continue for a few days, an opportunity may be found of verifying, or correcting, the result of my experiments.

I am, with the greatest respect,

Your's truly,

B. BEVAN.

Leighton Bussard.

Note by Dr. YOUNG.

It does not appear quite clear from reasoning, that the modulus ought to come out different in experiments on solids and on fluids: for though the linear compression in a fluid may be only $\frac{1}{3}$ as much as in a solid, yet the number of particles acting in any given section must be greater in the duplicate ratio of this compression, and ought apparently to make up the same resistance. And in a single experiment made hastily some years ago on the sound yielded by a piece of ice, the modulus did appear to be about 800,000 feet only: but the presumption of accuracy is the greater in this case the higher the modulus appears.

XXII. *Results of the application of Captain* Kater's *floating collimator to the astronomical circle at the observatory of Trinity College, Dublin, and remarks relative to those results. By the* Rev. J. Brinkley, *D.D. F.R.S P.R.I.A. Communicated by the Board of Longitude February* 2, 1826.

Read April 27, 1826.

The results of the observations which I am about to state will, I think, be considered as affording a strong testimony of the importance of the floating collimator. As applied to my circle, it furnishes the means of ascertaining the index correction with as great precision as by reversion, and in several points of view is undoubtedly superior to that method.

The reversing principle of the Dublin circle serves very conveniently for a measure of the accuracy of the floating collimator, and appears to show satisfactorily, that in the application of this instrument to any circle, no error belonging to the instrument will be introduced into the results; or in other words, that the results obtained by the assistance of the collimator, will not be sensibly affected by errors introduced by the collimator itself.

In the series of observations which I send, will be found—

I. The mean zenith distances of a number of stars, which were deduced solely by the application of the index correction, as determined by the collimator. In this way the circle was used as a mural circle; and as the observations were made both with the face of the circle east, and with the face west, the results may be considered the same as if they had been determined by two mural circles.

II. The inclinations of the line of collimation of the collimator, as determined on different days, are given. The permanency of this inclination from day to day is by no means necessary; but it taking place, appears to give additional value to the collimator; and the exactness with which it can be ascertained, puts beyond all question that the collimator is applicable to the most powerful instruments.

III. The index corrections of the circle, as determined on different days by the help of the collimator. The uniformity of these results is affected by the exactness of the adjustment of the vertical axis of the circle by the plumb-line; and possibly the line of collimation of the telescope of the circle, at least the index correction, may suffer occasional changes by a very small derangement in the parts of the instrument, so as to vibrate as it were about a mean state.

IV. The index corrections, as determined by reversing from several stars. These, as well as the index corrections determined by the collimator, are affected with the errors of division.

It may serve for illustrating the importance of the floating collimator, to consider that it furnishes the observer with a fixed star, which he can place in his horizon (in fact at any *given* altitude), unaffected by refraction, visible at *any instant*, and on which he can make his observations in a closed room. That this fictitious star can be observed with as great exactness as a fixed star of the first magnitude under the most favourable circumstances of daylight. I cannot but think, had such a discovery been announced, and the means of doing it not imparted at the same time, astronomers would have received it as one of the most important discoveries of modern times in this science, and anxiously expected its details.

What I have adduced in these tables of observations, will, I hope, assist in enabling others to appreciate the value of this new astronomical power, which, unless I deceive myself, may be considered as even belonging to a future more advanced state of practical astronomy.

Explanation of, and remarks on Table I.

The index corrections used in obtaining these results were found by the collimator. The zenith distances of the intersection of the wires of the collimator were found in the north and south positions. Half the sum of these is the inclination of the line of collimation of the collimator, and half the difference is the index correction. A mean of the index corrections, as determined on several days, has been used. This is not exactly the method suggested by Captain KATER; but the mean results must, in effect, be the same as if the correction had been determined for each observation. In making many observations with a large instrument, it would not be convenient, or perhaps advisable, to make for every star observed two cotemporaneous observations with the collimator. In this manner, however, a greater consistency in the separate results might be expected; but, as far as I have examined, there is no very marked superiority. That a judgment may be had, in three instances marked S, the results are deduced in this way; and certainly the agreement between the zenith distances of each day appears somewhat greater, than when the mean index correction is used; but still the final results are the same.

The zenith distances determined in the two positions of the circle differ by the effect of the errors of division of the circle. These affect both the observations of the star and of the collimator; and the quantity of error in the index correction arising from the errors of division, will always have a contrary effect in the east and west positions. This will account for the apparent diversity in some of the means of the zenith distances determined in the two positions. The greatest discordance is in the case of α Cygni. From thence it appears possible, that the error of a zenith distance determined by help of the collimator with the face of the circle in one position only may amount to $1\frac{1}{2}''$, arising from the errors of division in the Dublin circle. It is not probable that a greater error would be found to exist in any part of the circle; but the effect of errors of division in the index corrections, as determined by the collimator, necessarily disappears by taking the mean of the east and west results. Therefore the means of the two positions ought to exhibit the results the same as determined by the method of reversion. The comparison given in Table I. (notwithstanding some of the stars have been only observed a few times), proves in a most satisfactory manner the exactness to be obtained by the use of the collimator. Of the 20 stars, only two differ by $1''$ from the former determination, and these do not differ by more than $1\frac{1}{2}''$.

In Table I. *f. w.* signifies that the observations were made by the fine wire of the telescope, which is about $\frac{1}{3000}$ of an inch in diameter, and was kindly furnished me by Dr. WOLLASTON:

where these letters do not occur, the observations were made by a grosser wire.

The mean index corrections for this latter wire, as determined by the collimator, were as follow:

To Aug. 22 inclusive, .	Index correct. South of zenith	E	— 56″,30
		W	+ 54,46
From Aug. 25 to 29, ...		E	— 57,44
		W	+ 55,92
From Aug. 29 to Oct. 7, face W, ...			+ 59,90
From Oct. 7 to Nov. 10, face E, ...			— 59,11
From Nov. 10 to Dec. 3, face W, ...			+ 58,62
By the fine wire:			
From Oct. 15 to Nov. 10, face E, ...			— 15,10
From Nov. 10 to Dec. 3, face W, ...			+ 15,01

The interval between the wires is 1′ 13″,92; and therefore previously to Oct. 15, the index correction for the fine wire is found by subtracting the index correction for the thick wire from the interval 73″,92. From Oct. 15 the index correction for the fine wire was found directly.

Explanation of, and remarks on Table II.

This table gives the inclination of the line of collimation of the collimator to the horizon, as observed on different days. It is not necessary that the collimation of the collimator should remain permanent from day to day; but when it is found to do so, it stamps an additional value on the instrument. It is only necessary that derangement should not take place in carrying it from one box to the other. This might be feared, when we consider how small a change in the relative position of the parts of the instrument would disturb the observation. A displacement of the intersection of the cross wires of the collimator of only $\frac{1}{14000}$ of an inch, might occasion an error of 1″ in the inclination; so that it appears really wonderful that the collimation of the collimator has not for weeks together suffered perceptible derangement. The deviations from the mean are only such as may be safely attributed to the errors of observation and slight effects of change of temperature, except perhaps in one instance, the 27th July.

The determination of the inclination is independent on the position of the axis of the circle, and therefore also furnishes a criterion of the degree of exactness to which observations with the collimator may be made.

Explanation of, and remarks on Table III.

The index corrections given in this Table are those, by help of which the mean index corrections used for Table I. have been determined. It will be seen, that while the circle was frequently reversed (often many times in the same day), the corrections appear more variable than when the instrument remained for a length of time without being reversed. This was to be expected. An inexact adjustment of the vertical axis of the circle affects the index correction; and this is much more likely to take place when the circle is frequently reversed, than when not reversed. Indeed, from the reversals made for the purpose of trying the collimator, a source of error arose that was pointed out in the first instance by the collimator, and in so doing it furnished a strong testimony for itself.

It will be remarked, that in July and August the index corrections for the face east differ from those for the face west. This is the circumstance alluded to, and requires explanation.

After the circle is reversed, the plumb-line adjustment is made, if necessary; and then the index correction ought to be precisely the same in quantity as before reversal, provided the parts of the instrument or the plumb line have suffered no derangement in the reversion. I had formerly particularly attended to this circumstance, which I was enabled to do by means of 4 gold dots placed by the maker on the face of the circle. The plumb-line having been made to bisect the adjusting dot on the *frame* of the circle, a gold dot on the lower part of the circle was also bisected by the plumb-line by moving the circle. The circle was then reversed; and if when the dot on the frame was bisected by the plumb-line the dot on the circle was also bisected by the plumb-line, the circle had turned without derangement, at least the plumb-line had suffered no derangement as to its point of suspension. The examination of this is tedious and delicate; and having satisfied myself that no error was to be feared, it was not usually attended to. However, when the observations with the collimator were commenced, it was soon found that the index correction appeared greater on the east side than on the west; and therefore that probably, in the act of reversing, a small derangement about the point of suspension of the plumb-line took place, less than a second in its mean quantity. This was fully confirmed from the examination by the gold dots. The result of this would have been to have made the zenith distances south of the zenith, when computed from the east and west readings, appear greater than they ought; whereas, not regarding the plumb-line as to reversal, and using the collimator, the true results are obtained. This accounts for the inequality of the index corrections in the two positions of the circle.

By precautions since taken, the circle now appears to turn without being subject to this inconvenience. It is only an inconvenience, because the quantity of correction is obtained by the gold dots; but not so conveniently or so satisfactorily as by the collimator. Latterly however the reversals have been but rare, as I considered it desirable to continue the circle for some time together in the same position, that the collimator might be tried under various circumstances. The steadiness of the line of collimation of the circle, and of the line of collimation of the collimator in the periods in which the circle remained in the same position, is remarkable.

Explanation and remarks on Table IV.

This Table shows the index error obtained by observing the same star in the east and west positions of the face of the circle. It is given to show the general agreement with the same determined by the collimator, taking a mean between the results by the collimator on the east and west sides. But this is not the accurate quantity to be applied to each observation, as will be understood by what was stated in the explanation of Table III. This fourth table also serves to show that there is great uniformity between the adjacent quadrants of the Dublin circle.

Additional remarks on Table I.

Some of the discordances of the east and west results may appear greater than would have been expected; but it is to be considered that each is a single observation, not the mean of two; and with reference to the collimator, the errors of division have necessarily, and may also with respect to the star, have contrary effects. Add to these the sources of error arising from inexact bisection of the plumb-line dot, from the bisection of the star from reading off; and, in so large an instrument as the Dublin circle, from the effects of unequal temperature, to which may be also added the irregularities of refraction; and then we may rather wonder greater discordances are not sometimes to be found. The deviation from the mean very rarely indeed mounts to 5″; and it is to be remembered, that a second in the circle is only about $\frac{1}{4000}$ of an inch. It may be objected, that with reference to mural circles, in which the reversing principle cannot be applied, the effect of the errors of division on the index correction cannot be done away. In mural circles the principal means of obtaining the index correction are observations of the pole star above and below the pole, and by observing by reflection. The difficulties that occur in the practice of these two methods, when contrasted with the use of the collimator, are surely not compensated by the advantage of obtaining perhaps the index correction more free from the errors of division. When we consider Mr. TROUGHTON'S improved method of dividing, and the application of six microscopes to a mural circle, we need not fear any appreciable error in determining by the floating collimator the index correction. Besides, as Captain KATER has pointed out, we may by changing the inclination of the collimators bring into use different parts of the circle, and so avoid the effect of errors of division.

By a comparison of the inclinations of the line of collimation of the collimator, as determined in October and November by both the fine wire and thick wire, it will be seen that, as far as errors of observation are concerned, an error of a second rarely takes place. It may therefore serve to illustrate the powerful optical principle, on which the application of the collimator is founded, to remark that, as the collimator furnished by the Board of Longitude is only about 18 inches long, a space of less than $\frac{1}{20000}$ of an inch is ascertained; for to such a degree of exactness the position of the cross wires of the collimator can be ascertained. Thus perhaps a most severe test of a micrometer screw might be furnished.

J. BRINKLEY.

Table I.

Mean zenith distances reduced to Jan. 1st, 1825, found by applying the index correction, as determined by the collimator, to the observed zenith distances, and reducing them by the equations.

	Face of Circle.	
	East.	West.
1825.	α Cassiopeæ.	
Oct. 8	2 11 21,79	
9	21,67	
12	19,12	
14	22,35	
15	22,63	
21	20,99	
25	20,06	
Nov. 3	20,61	
7	23,32	
9	20,88	
11	....	2 11 22,76
14	....	21,56
16	....	21,86
17	....	22,93
24	....	22,50
25	....	22,24
Means	2 11 21,34	2 11 22,31
		21,34
	Mean	2 11 21,82
	By Catalogue	22,14

Using single observations of collimator made nearest to the time of observation of the star.

Oct. 8	2 11 21,89	
9	21,77	
12	19,81	
S 14	21,54	
15	21,87	
21	21,48	
25	20,95	
Nov. 3	20,38	
7	23,31	
9	22,70	
11	....	2 11 22,38
14	....	21,48
16	....	21,32
17	....	22,39
24	....	22,42
25	....	22,16
Means	2 11 21,57	2 11 22,03
		21,57
	Mean	2 11 21,80

	Face of Circle.	
	East.	West.
1825.	Aldebaran.	
Aug. 5	37 14 15,80	37 14 15,21
6	14,48	14,81
7	14,67	13,86
Means	37 14 14,98	37 14 14,63
		14,98
	Mean	37 14 14,80
	By Catalogue	15,41
1825.	Capella.	
July 25	....	7 34 38,52
29	....	40,80
30	7 34 41,60	
Aug. 4	40,96	
5	....	38,47
6	40,58	
7	....	36,92
9	....	37,63
Means	7 34 41,05	7 34 38,47
		41,05
	Mean	7 34 39,76
	By Catalogue	40,72
1825.	β Tauri.	
Aug. 5	....	24 56 10,51
6	24 56 11,65	
7	....	24 56 10,89
		24 56 10,70
		11,65
	Mean	24 56 11,17
	By Catalogue	12,76
1825.	α Orionis.	
July 25	....	46 1 14,88
29	....	14,66
30	46 1 14,76	
Aug. 5	12,11	13,81
7	13,64	12,98
8	13,84	
10	11,94	
Means	46 1 13,26	46 1 14,08
		13,26
	Mean	46 1 13,67
	By Catalogue	14,95

	Face of Circle.	
	East.	West.
1825.	Sirius.	
July 24	69 52 14,07	
25	....	69 52 12,29
27	11,22	
Aug. 22	11,27	9,59
Sept. 3	....	9,03
Means	69 52 12,19	69 52 10,30
		12,19
	Mean	69 52 11,24
	By Catalogue	10,87
1825.	Procyon.	
Aug. 5	47 43 13,01	
22	13,75	47 43 14,11
Sept. 3	f. w.......	14,52
	47 43 13,38	47 43 14,31
		13,38
	Mean	47 43 13,84
	By Catalogue	13,97
	Arcturus.	
July 28	33 17 20,57	
29	....	33 17 20,62
Aug. 5	19,08	
Sept. 5	....	21,84
14	f. w......	20,83
21	f. w......	20,92
29	f. w......	22,55
Oct. 7	f. w......	24,94
21	21,45	
Nov. 5	f. w. 18,79	
9	f. w. 21,59	
10	f. w......	21,94
18	f. w......	22,61
29	f. w......	22,15
Dec. 2	f. w......	22,77
3	....	21,47
Means	33 17 20,30	33 17 22,06
		20,30
	Mean	33 17 21,18
	By Catalogue	21,03

Table I.—*continued.*

Mean zenith distances reduced to Jan. 1st, 1825, found by applying the index correction, as determined by the collimator, to the observed zenith distances, and reducing them by the equations.

	Face of Circle.	
	East.	West.
	α Cor. bor.	
1825.		
Aug. 22	26 4 39,31	26 4 40,24
23	36,09	39,75
29	f. w.......	40,73
31	f. w.......	39,73
Means	26 4 37,70	26 4 40,11
		37,70
	Mean	26 4 38,90
	By Catalogue	38,63

	Face of Circle.	
	East.	West.
1825.	α Serpentis.	
Aug. 29	f. w.......	46 24 14,59
31	f. w.......	13,30
Sept. 5	f. w.......	14,70
	Mean	46 24 14,20
	By Catalogue	13,59

	Face of Circle.	
	East.	West.
	α Herculis.	
		f. w.
Aug. 29	....	38 47 22,74
30	....	22,59
31	....	23,28
Sept. 5	....	24,28
6	....	25,22
7	....	22,66
8	....	24,61
10	....	25,01
22	....	22,28
	Mean	38 47 23,63
	By Catalogue	23,15

	Face of Circle.	
	East.	West.
1825.	α Ophiuchi.	f. w.
	....	40 41 30,16
Aug. 29	....	30,07
30	....	29,26
31	....	29,62
Sept. 5	....	31,43
6	....	31,42
10	....	30,30
14	....	29,09
22	40 41 33,53	
Nov. 4	30,71	
9		
Means	40 41 32,12	40 41 30,17
		32,12
	Mean	40 41 31,14
	By Catalogue	30,17

	Face of Circle.	
	East.	West.
1825.	γ Draconis.	
July 24	1 52 26,63	
25	23,74	
28	....	1 52 22,89
29	....	23,68
Aug. 2	....	22,76
17	27,28	
18	....	24,08
21	24,22	
29	....	23,29
31	....	24,05
Sept. 5	....	23,59
10	....	21,33
12	....	23,75
14	....	22,18
17	....	22,36
22	....	23,34
28	....	25,48
Oct. 4	....	24,69
21	f. w. 26,75	
Nov. 4	f. w. 26,95	
7	f. w. 26,73	
9	f. w. 26,22	
16	f. w.......	26,53
17	f. w.......	26,11
19	f. w.......	26,40
25	f. w.......	25,89
Dec. 2	f. w.......	26,80
Means	1 52 26,07	1 52 24,17
		26,07
	Mean	1 52 25,12
	By Catalogue	25,11

	Face of Circle.	
	East.	West.
1825.	α Lyræ.	
July 2	14 45 36,23	14 45 36,77
15	35,48	37,24
16	37,33	34,91
17	36,67	38,47
18	39,18	37,14
19	39,46	36,08
25	37,87	
27	....	38,00
28	. 40,90	
29	....	36,21
Aug. 2	....	36,91
6	38,00	
10	37,40	39,17
17	40,50	38,59
18	38,02	37,66
21	40,71	37,38
22	37,67	39,83
24	39,00	39,20
25	39,18	39,40
29	....	39,26
30	....	38,08
31	....	37,81
Sept. 5	....	34,98
6	....	37,55
8	....	37,46
12	....	37,29
14	....	38,41
16	....	37,30
22	....	34,85
27	....	41,06
28	....	40,38
30	....	39,28
Oct. 14	41,76	
17	40,09	
21	40,26	
Nov. 4	40,54	
7	40,46	
9	39,54	
11	....	38,56
12	....	39,55
16	....	40,07
25	....	40,53
Dec. 2	....	40,66
3	....	40,42
Means	14 45 38,92	14 45 38,18
		38,92
	Mean	14 48 38,55
	By Catalogue	38 32

Mean zenith distances reduced to Jan. 1st, 1825, found by applying the index correction, as determined by the collimator, to the observed zenith distances, and reducing them by the equations.

Table I.	Face of Circle.	
	East.	West.
1825.	α Aquilæ.	
Oct. 7	44 58 24,03	
14	21,02	
15	24,78	
17	21,77	
19	23,13	
29	24,48	
Nov. 4	24,69	
7	24,52	
9	23,87	
10	25,02	
12	....	44 58 24,83
15	....	25,90
Dec. 2	....	26,93
3	....	25,62
Means	44 58 23,73	44 58 25,82
		23,73
	Mean	44 58 24,77
	By Catalogue	24,26
1825.	α Cygni.	
Aug. 10	8 43 38,64	8 43 39,12
29	....	37,41
31	....	34,92
Sept. 3	....	39,12
4	....	37,83
6	....	36,89
10	....	38,64
13	....	37,49
22	....	36,51
26	....	37,69
29	....	39,18
Oct. 15	41,78	
25	41,36	
29	41,21	
4	41,02	
7	40,85	
9	43,49	
10	42,27	
11	....	38,19
12	....	38,87
14	....	38,19
16	....	39,42
17	....	39,16
Dec. 2	....	40,44
3	....	39,00
Means	8 43 41,33	8 43 38,23
		41,33
	Mean	8 43 39,78
	By Catalogue	40,26

	Face of Circle.	
	East.	West.
Using single observation of collimator made nearest to the time of ob. of the star.		
Aug 10	....	8 43 38,26
29	....	38,21
31	....	35,03
Sept. 3	....	37,84
4	....	36,55
6	....	37,71
10	....	38,19
13	....	39,12
22	....	38,87
26	....	38,32
29	....	37,57
Oct. 15	8 43 42,54	
25	40,41	
29	42,23	
Nov. 4	41,25	
7	40,86	
9	41,67	
10	40,05	
11	....	38,57
12	....	38,87
14	....	38,27
16	....	39,96
17	....	39,70
Dec. 2	....	40,62
3	....	38,28
Means	8 43 41,29	8 43 38,33
		41,29
	Mean	8 43 39,81
	By Catalogue	40,26
	2 α Capricorni.	
Aug. 29	....	66 27 58,55
31	....	57,41
Sept. 3	....	54,04
4	....	57,38
6	....	56,92
10	....	59,12
11	....	60,51
13	....	55,62
22	....	59,70
26	....	62,28
27	....	60,18
28	....	59,02
Oct. 4	....	59,12

S

	Face of Circle.	
	East.	West.
1825.	2 α Capricorni—*continued.*	
Oct. 7	66 27 58,61	
14	58,06	
15	60,28	
17	58,04	
29	58,40	
Nov. 4	59,44	
7	59,20	
9	60,74	
10	59,37	
12	....	60,65
Dec. 2	f. w......	63,57
Means	66 27 59,13	66 27 58,94
		59,13
	Mean	66 27 59,03
	By Catalogue	59,11
1825.	α Pegasi.	
Aug. 29	....	39 7 14,89
Sept. 27	....	17,35
28	....	17,34
29	....	16,55
30	....	16,97
Oct. 7	39 7 17,47	
8	16,10	
12	19,10	
13	18,29	
14	16,10	
21	17,53	
25	17,07	
29	14,94	
Nov. 3	16,52	
4	15,84	
7	15,36	
9	18,10	
10	17,87	
11	....	15,79
16	....	16,50
Dec. 3	....	13,83
Means	39 7 16,95	39 7 16,15
		16,95
	Mean	39 7 16,55
	By Catalogue	16,22

Mean zenith distances reduced to Jan. 1st, 1825, found by applying the index correction, as determined by the collimator, to the observed zenith distances, and reducing them by the equations.

Tab. I.	Face of Circle. East.	Face of Circle. West.
1825.	α Andromedæ.	
Oct. 8	25 15 45,78	
14	44,52	
21	46,22	
25	46,60	
Nov. 3	47,07	
4	46,70	
7	45,55	
9	46,71	
10	47,80	
11	....	25 15 44,63
14	....	45,14
16	....	46,17
17	....	45,05
24	....	46,17
25	....	46,04
Means	25 15 46,32	25 15 45,63 46,32
	Mean By Catalogue	25 15 45,92 45,60

Using single observation of collimator made nearest to the time of observation of the star.

	East.	West.
Oct. 8	25 15 45,65	
14	45,35	
21	46,43	
25	45,71	
Nov. 3	47,30	
4	46,93	
7	45,56	
9	44,89	
10	46,58	
11	....	25 15 45,01
14	....	45,22
16	....	46,76
17	....	45,59
24	....	46,25
25	....	46,12
Means	25 15 46,04	25 15 45,82 46,04
	Mean By Catalogue	25 15 45,93 45,60

	Face of Circle. East.	Face of Circle. West.
1825.	Polaris.	
Oct. 7	34 59 17,44	
f. w.	18,51	
8	17,88	
f. w.	18,37	
15	18,98	
f. w.	18,85	
21	18,56	
f. w.	18,83	
Nov. 7	18,63	
f. w.	19,78	
9	17,87	
f. w.	18,60	
11	....	34 59 19,69
f. w.	....	16,92
14	....	18,94
f. w.	....	17,71
17	....	18,80
f. w.	....	17,53
Means	34 59 18,52	34 59 18,26 18,52
	Mean By Catalogue	34 59 18,39 18,06

	East.	West.
1825.	Polaris, S. P.	
Aug. 22	38 14 16,05	38 14 13,23
12	14,75	
f. w.	15,34	
20	15,09	
f. w.	16,19	
28	16,26	
f. w.	16,43	
Nov. 10	15,30	
f. w.	15,59	
10	....	14,75
f. w.	....	13,18
11	....	14,46
f. w.	....	13,75
18	....	17,25
f. w.	....	16,40
Means	38 14 15,67	38 14 14,72 15,67
	Mean By Catalogue	38 14 15,20 14,94

Co. lat. by Polaris,
36 36 46,8.

	North pole dist. Jan. 1, 1825, by collimator.			N. P. dist. by Catalogue.	Diff.
	°	′	″	″	
α Cassiopæ ..	34	25	24,68	24,36	+ 0,32
Aldebaran ..	73	51	1,30	1,91	— 0,61
Capella......	44	11	26,26	27,22	— 0,96
β Tauri	61	32	57,67	59,26	— 1,59
α Orionis....	82	38	0,17	1,45	— 1,28
Sirius	106	28	57,74	57,37	+ 0,37
Procyon.....	84	20	0,34	0,47	— 0,13
Arcturus	69	54	7,68	7,53	+ 0,15
α Cor. bor. ..	62	41	25,40	25,13	+ 0,27
α Serpentis...	83	1	0,70	0,09	+ 0,61
α Herculis ...	75	24	10,13	9,65	+ 0,48
α Ophiuchi...	77	18	17,64	16,67	+ 0,97
γ Draconis...	38	29	11,62	11,61	+ 0,01
α Lyræ......	51	22	25,05	24,82	+ 0,23
α Aquilæ	81	35	11,27	10,76	+ 0,51
α Cygni......	45	20	26,28	26,76	— 0,48
2 α Capricorni	103	4	45,53	45,61	— 0,08
α Pegasi.....	75	44	3,05	2,72	+ 0,33
α Andromedæ	61	52	32,43	32,10	+ 0,33
Polaris	1	37	28,40	28,44	— 0,04
				Mean diff.	— 0,03

The above polar distances are deduced from the zenith distances by applying the co-lat. 36 36 46,5; and the catalogue referred to is mine of 1823, given in the Phil. Trans. 1824.

J. BRINKLEY.

Table II.

	Inclination of Collimator.	
	Face East.	Face West.
	′ ″	′ ″
July 22	7 10,00	7 9,80
24	10,15	9,08
25	9,74	10,43
27	10,00	6,80
28	10,12	11,79
P. M.	10,22	9,48
30	8,89	11,80
31	9,06	9,60
Aug. 2	……	10,71
3	11,37	12,47
4	11,47	
5	……	9,52
6	10,31	
7	9,56	
8	……	11,36
9	……	11,36
P. M.	……	10,89
10	8,61	
P. M.	10,85	
11	……	11,40

Several days of excessive high wind shaking the walls of the room on which the collimator was supported.

	Face East.	Face West.
Aug. 16	10,62	
17	10,22	
18	……	10,30
19	……	10,78
20	……	11,01
21	11,00	
Means	7 10,13	7 10,47

Cleaned mercury, and at same time cleaned bottom of collimator, which may have caused a small change in the inclination.

	Face East.	Face West.
Aug. 26	11,01	11,48
27	11,85	12,43
28	12,13	12,80
29	11,07	12,00
P.M.	12,16	

From this time the circle remained in the same position for a considerable interval.

	Inclination of Collimator.
	Face West.
	′ ″
Aug. 29	7 12,50
30	14,06
31	13,40
P. M.	14,28
Sept. 1	12,75
2	12,68
P. M.	13,60
3	11,94
4	11,38
5	9,45
6	12,45
7	12,18
8	12,10

Two days of storm.

	Face West.
10	12,88
11	12,35
14	12,03
21	12,93
22	10,99
27	8,83
28	10,49
Oct. 1	11,26
7	11,16
Mean	7 12,08

Reversed circle.

Circle turned without derangement of plumb-line, as ascertained by gold dot.

Face now East.

		Fine Wire.
		″
Oct. 7	7 10,81	
13	11,07	
14	10,40	
15	10,62	10,40
17	11,67	11,33
21	9,93	10,00
22	8,60	9,12
24	10,48	10,21
27	10,57	10,98
28	11,43	11,92
29	11,58	11,50
Nov. 4	12,03	11,46
7	9,97	8,86
9	9,07	10,20
11	8,56	8,36
Means	7 10,45	10,36

Reversed circle.

	Inclination of Collimator. Face West.	
	Thick Wire.	Fine Wire.
	′ ″	″
Nov. 11	7 9,97	9,66
14	11,55	11,53
P. M.	9,53	10,52
17	10,06	10,77
19	10,16	10,72
25	10,93	10,90
30	11,91	12,11
Dec. 2	9,38	8,71

Cleaned mercury.

	Thick Wire.	Fine Wire.
3	12,03	12,05
Means	7 10,61	10,77

Table III.

	Index Correction.	
	Face East.	Face West.
1825.	″	″
July 22	54,87	54,80
24	58,02	54,64
25	55,65	54,26
27	56,35	53,81
28	56,04	53,18
P. M.	59,28	55,72
30	55,31	52,98
31	59,16	52,90
*Aug. 2	……	55,37
3	56,40	54,96
4	55,20	
5	……	54,89
6	54,32	
7	53,93	
8	……	54,95
9	……	55,50
P. M.	……	55,93
10	56,38	
P. M.	56,26	
11	……	53,60

Several days of high wind.

16	55,40	
17	58,02	
18	……	55,20
19	……	53,50
20	……	54,08
21	56,50	
Means	−56,30	+54,46

Adjusted plumb-line dot, by which the index correction is necessarily affected. Cleaned mercury, &c.

Aug. 26	59,08	56,11
27	56,58	56,56
28	57,80	55,23
29	56,30	55,80
Means	−57,44	+55,92

	Index correction.	
	Face East.	Face West.

Took off plumb-line, and cleaned notch.

Aug. 29 P.M.	61,36	

Reversed circle, which remained with face west till Oct. 7.

Aug. 29 P.M.	……	60,70
30	……	60,44
31	……	60,77
P.M.	……	60,01
Sept. 1	……	60,25
2	……	58,92
P.M.	……	59,77
3	……	59,97
4	……	58,62
5	……	58,98
6	……	60,72
7	……	59,08
8	……	59,70

2 days of storm.

10	……	59,45
11	……	60,18
14	……	61,53
21	……	59,91
22	……	62,28
27	……	60,53
28	……	58,29
Oct. 1	……	58,57
7	……	59,03
	Mean	+59,90

	Index correction.	
	Thick Wire.	Fine Wire.

Reversed Circle, face now East.

Oct. 7	59,21	
13	59,80	
14	58,30	
15	58,35	15,63
17	57,84	15,70
21	58,90	14,67
22	59,60	14,65
24	58,58	15,28
27	60,00	15,15
28	58,76	15,85
29	58,09	16,08
Nov. 4	58,88	14,78
7	59,10	15,33
9	60,93	13,87
11	60,33	14,23
Means	−59,11	+15,10

Reversed Circle, face now West.

Nov. 11	59,00	14,98
14	58,62	14,80
P. M.	58,70	14,86
17	59,16	14,70
19	58,86	14,75
25	58,70	14,67
30	57,88	15,61
Dec. 2	58,80	15,18

Cleaned Mercury.

2	57,90	15,55
Means	+58,62	−15,01

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* From this time the collimator was always lifted from the mercury, and replaced before the observation.

Table IV.

Index corrections by reversion to Aug. 21 inclusive.		
		"
α Lyræ	13 Ob. East, and 13 West	55,80
γ Draconis	2 E 3 W	56,41
Arcturus	1 W 1 E	55,36
Aldebaran	3 W 3 E	55,55
β Tauri	2 W 1 E	55,85
α Orionis	4 W 3 E	55,20
Capellæ	5 W 3 E	56,67
α Cor. bor	2 W 2 E	55,53
Sirius	1 W 1 E	56,22
Procyon	1 W 1 E	55,20
α Cygni	1 W 1 E	55,10
Mean by Collimators E 56,30 } 55,38 W 54,46 }	Mean...	55,72

XXIII. *On the means of facilitating the observation of distant stations in geodætical operations. By Lieutenant* THOMAS DRUMMOND, *of the Royal Engineers. Communicated April* 14, 1826, *by Lieut. Colonel* H. COLBY, *F. R. S.*

Read May 4, 1826.

IN consequence of the Report of a Select Committee of the House of Commons in June 1824, it was resolved, that a new Survey of Ireland should be immediately undertaken.

The necessity of carrying on this extensive work with the utmost degree of rapidity, consistent with accuracy of execution, being strongly urged in the Report to which I have alluded, the arrangements were directed to be made on a suitable scale, and every method to be adopted that seemed likely to contribute to this end. The triangulation, as forming the basis of the survey, and the means of accelerating its execution, claimed immediate attention; and I was directed by Colonel COLBY, at that time actively engaged in making the necessary preparations for this important undertaking, to consider by what means our distant stations might be rendered more frequently observable than the state of the atmosphere usually permits.

From this source, at least in this country, arises the chief delay in carrying on such operations; and the experience of a previous season among the Western Islands, had shown the probability of this impediment being materially increased, and more than ordinary difficulty encountered, in effecting the triangulation of Ireland, and in connecting that country with the western shores of Scotland and England. The connecting triangles are large, many of the sides being 60, 70, and 80 miles. The means resorted to for rendering these distant stations visible in ordinary states of the weather, and their successful application on some occasions of difficulty that presented themselves at the close of last year's operations, form the subject of the communication which I have now the honour of submitting to this Society.

The reflection of the sun from a plane mirror, as affording a point of observation that might be seen at remote distances, was suggested and employed by Professor GAUSS in 1822, while engaged with a trigonometrical measurement in Hanover; and the result of the first trials made at Inselberg and Hohenhagen, rendered it highly probable that it might be applied with much advantage to this purpose.

The principle was adopted in this country when Colonel COLBY and Captain KATER were engaged, in 1822, in verifying General ROY's triangulation connecting the meridians of Paris and Greenwich. At their concluding station on Shooter's Hill, seven or eight days had elapsed, during which Hanger Hill Tower, though only 10 miles distant, having remained completely obscured by the dense smoke of London, tin plates were attached to the signal post, so as to reflect the sun towards the station at stated times on a certain day.

At the hours for which they had been calculated these plates became visible, and the observations were in consequence immediately and easily completed. In the subsequent operations of 1823, recourse was again had on two important occasions to the same method, and with equal success. I allude to Leith Hill, near Dorking, in Surrey, and Wrotham Hill, in Kent, stations which it was of the utmost consequence to observe from Berkhampstead Tower, near Hertford. Our efforts to effect these observations having for some time been rendered unavailing by the thick mist, so frequently overhanging the bed of the Thames, a series of bright tin plates was put up on both stations. Each set consisting of six or eight plates, was attached to a smooth flat board, placed vertically by the plumb line, and turning on a pivot: the respective inclinations of the plates with the face of the board being determined, so that they might have the positions required for reflecting, in succession, the sun's rays towards Berkhampstead tower, when the surface of the board was turned at right angles to the line of direction. Although this method admitted but of rude execution, it fully answered the purpose for which it was employed: the plates became visible in succession at the appointed hours, the duration of each varying with the inequality of its surface, but being generally from ten to fifteen minutes; they were seen nearly at the same hours for some days before and after that for which they were calculated.

The distance to Leith Hill is 45 miles, and the observations were in this way completed without the hill itself having been visible during the whole of our stay, which was nearly three weeks.

The utility of employing the sun's reflection as a point of observation being established by the result of these experiments, it only remained, instead of a temporary expedient, rather difficult of execution, to substitute an instrument that might be used on all occasions, simple

Fig. 1

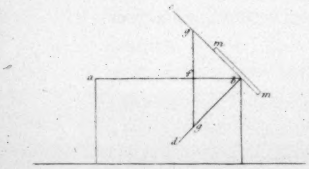
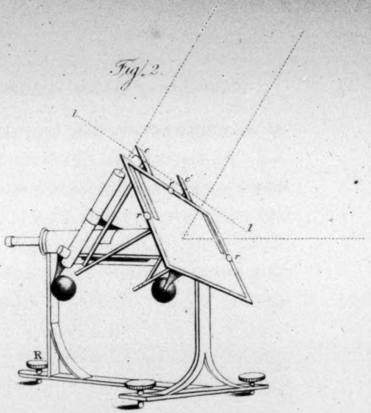
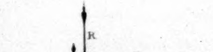


Fig. 2



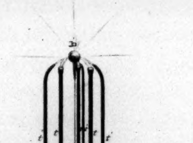
One Inch to One Foot.

Fig. 3



One Inch to Eighteen Inches.

Fig. 4



One Inch to Four Inches.

J. Basire sc.

in its construction, and easy of management. Fig. 1, Plate XII. represents an instrument contrived with this view, which was employed last year in Ireland with much advantage. *ab* is a telescope of 12 inches focal length, and serves as the axis of the instrument; the bars *bd* and *bc* form a right angle; and the bar *gg*, placed so that *bf* (fig. 2), shall be equal to *fg*, works between *bd* and *bc*, carrying a small telescope, such as is usually attached to sextants, and provided with a rectangular eye-piece. The mirror *mm*, of which different sizes may be used according to circumstances, is connected with the instrument by three adjusting screws *r*. The bars *bc*, *b′c′*, being now made to coincide with *ab*, a moveable spirit level is placed across them in the position *ll*, fig. 2, and rendered horizontal by the foot screws; by the same means the axis *ab*, to which a level is permanently attached, is also brought into a horizontal position. The moveable spirit level being now transferred to the surface of the mirror, the three adjusting screws *r*, are employed to render it horizontal. The mirror will then be parallel to *ab* and *ll*, and will have the required position on the instrument. The telescope *ab* being now directed upon the object to which the reflection is to be thrown, and the small telescope *gg* turned towards the sun, its rays will then be reflected parallel to the axis of the instrument *ab*. The head of the screw R, fig. 2, is graduated, so that by means of it and the spirit level attached to the axis, the required elevation or depression may be given to the instrument when the object towards which it is directed happens to be invisible, its direction only being known relatively to some nearer object; and which, it may be remarked, has been the case in every instance in which it has been employed on the survey. When packed for travelling, the mirror *mm* is detached, and the bar *gg* turned till it coincides with *bf*. The instrument once directed, its management was usually confided to one of the non-commissioned officers.

To combine, with the heliostat now described, a means of exhibiting a bright light at night, that no opportunity might be lost of effecting our purpose, was the next consideration.

In the beginning of the survey, General ROY had, on several occasions, but especially in carrying his triangles across the Channel to the French coast, made use of Bengal and white lights prepared at the Royal Arsenal: for these, parabolic reflectors, similar to those with which our light-houses are supplied, and illuminated by argand burners, were afterwards substituted, as more convenient; but they have been gradually discontinued, the advantages derived from them proving inadequate, from their want of power, to the trouble and expense incident to their employment. In the trigonometrical operations of 1821 carried on by Colonel COLBY and Captain KATER, conjointly with MM. ARAGO and MATHIEU, for connecting the meridians of Greenwich and Paris, an apparatus of a very different kind was employed for the first time. A large plano-convex lens 0.76 metre square being substituted for a parabolic reflector, and the illuminating body an argand lamp with four concentric wicks. The lens was composed of a series of concentric rings, reduced in thickness and cemented together at the edges. This apparatus resulted from an inquiry into the state of the French light-houses, and was prepared under the direction of MM. FRESNEL and ARAGO. Its construction and advantages are detailed in a "Mémoire sur un nouveau systême d'éclairage" by M. Fresnel. The light which it gave is stated to possess 3¼ times the intensity of that given by a reflector. It was employed, during the operations alluded to, at Fairlight Down and Folkestone Hill on the English coast; at Cape Blancnez and Montlambert on the French coast: the greatest distance at which it was observed being 48 miles; and its appearance, I have understood from Colonel Colby, was very brilliant.

But valuable as this apparatus may be when employed in a light-house, the purpose for which it was indeed invented and constructed, the properties of the simple parabolic reflector appeared still to give it a preference for the service of the Trigonometrical Survey, provided a more powerful light could be substituted in its focus, instead of the common argand lamp.

With this object in view, I at first endeavoured to make use of some of the most brilliant pyrotechnical preparations, then phosphorus burning in oxygen, with a contrivance to carry off the fumes of phosphoric acid was tried: but the first attempts with these substances promising but little success, they were abandoned. In all cases the flames, besides being difficult and troublesome to regulate, were large and unsteady; little adapted to the nature of a reflecting figure, which should obviously, when used to the utmost advantage, be lighted by a luminous sphere, the size being regulated by the spread required to be given to the light. This form of the focal light it was manifest, either could not be obtained or preserved when combustion was the source of light; and it was chiefly this consideration which then led me to attempt applying, to the purpose in view, the brilliant light emanating from several of the earths when exposed to a high temperature; and at length I had the satisfaction of having an apparatus completed, by which a light so intense was produced, that when placed in the focus of a reflector the eye could with difficulty support its splendour, even at the distance of forty feet, the contour of the reflector being lost in the brilliancy of the radiation.

To obtain the requisite temperature, I had recourse to the known effect of a stream of oxygen directed through the flame of alcohol*, as a source of heat free from danger, easily procured and regulated, and of great intensity. Fig. 4 represents the apparatus such as it is now made for the survey. The spirit entering at *a*, ascends through the tubes *t*, while the oxygen entering at *d* is directed by the jets *t′* upon the small ball of lime *b*, the tubes *t′* are connected with the cylindrical box *h* by flexible caoutchouc tubes *e f*, and also pass with friction through small cylinders at *c*, which admit of being moved backwards and forwards upon the arms, and are clamped when in the proper position by small millheaded screws at the sides. By these means every requisite adjustment is obtained for the jets through which the gas issues. The apparatus is attached by its base to the stand which carries the reflector (fig. 3, Plate XII.) and the small ball may then, by means of the horizontal and vertical screws *r*, be brought with great accuracy into the focus of the reflector. The cistern *c* containing the alcohol is placed behind the reflector (fig. 3,) and being connected with the stem *a* by a flexible caoutchouc tube, may be elevated or depressed on the upright rod *r* fig. 3, and the flame of the spirit accordingly regulated so as to produce the greatest effect. A flexible tube leads from *d* to the vessel containing the oxygen, which may be either a common gas-holder, or perhaps a silk bag with a layer of caoutchouc, such as they are now made, might be conveniently employed for this purpose. The apparatus first made was provided with five jets, and could light up a ball $\frac{3}{8}$ inch in diameter; that now represented has only three, and with it a ball $\frac{1}{4}$ of an inch in diameter may be used sufficiently large to admit of the requisite allowance being made for aberration in the reflector from its true figure, as well as uncertainty of direction arising from terrestrial refraction.

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* Annals of Philosophy, vol. ii. p. 99.

To ascertain the relative intensities of the different incandescent substances that might be employed, they were referred, by the method of shadows, to an argand lamp as a common standard; the light from the brightest part of the flame being transmitted through apertures, equal in diameter to the small spheres of the different substances submitted to experiment.

The result of several trials made at the commencement gave for

Lime . . . . . 37 times
Zirconia[*] . . . 31 times
Magnesia . . . 16 times the intensity of

an argand burner. The oxide of zinc was also tried; but besides wasting away rapidly, it proved inferior even to magnesia.

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[*] From the description given by Berzelius of the light emitted by zirconia before the common blow-pipe, a different result might have been expected.

Of these substances, and also of their compounds with one another, lime appearing to possess a decided superiority, my subsequent experiments were confined to it alone, and by a more perfect adjustment of the apparatus, by bringing the maximum heat, which is confined within narrow limits, exactly to the surface of the ball, and by using smaller balls than those employed in the early experiments, a very material increase of light has been obtained. The mean of 10 experiments, made lately with every precaution, gives for the light emitted by lime, when exposed to this intense heat, 83 times the intensity of the brightest part of the flame of an argand burner of the best construction, and supplied with the finest oil. The lime from chalk, and such as is known at the London wharfs by the name of flame lime, appears to be more brilliant than any that has been tried.

When well burned Carrara marble is made into a paste with water, and gradually dried, it appears to be nearly equal to the preceding: when strongly compressed, or very porous, in both cases it is inferior.

The lime from the chalk, besides being the most brilliant, is in other respects very convenient for use; it admits of being turned in the lathe, and thus any number of the small focal balls, with slender stems attached to them, may be prepared with the utmost facility, uniform in size, and perfect in figure. The surface of the ball, by the continued action of the heat, appears to be kept nearly in a state of fusion; it is gradually worn down; and, on cooling, presents a semi-crystalline appearance. Being desirous of ascertaining what effect so intense a light would have on a mixture of chlorine and hydrogen, two tubes 10 inches long, with bulbs about two inches in diameter blown at one extremity, were filled with a mixture of these gases, and placed within two inches of the luminous ball of lime, which was $\frac{3}{8}$ of an inch in diameter. The additional light, reflected by a concave mirror $9\frac{1}{2}$ inches in diameter, was also concentrated on one of the balls.

After some minutes exposure to the light the glass began to get dim, especially on the sides nearest the light, being covered with a thin whitish film; at the end of 15 minutes they were withdrawn, and the stop-cocks being opened, under water coloured with an infusion of litmus, it immediately rushed in, filling one-third of the ball on which the reflected light had also been concentrated, and one-half of the other exposed to the direct light only: the difference probably arising from some accidental inequality in the brightness of the opposite sides of the ball. The purple tint of the litmus was rapidly changed to red, which, again by the action of the uncombined portion of chlorine, gradually faded away, and was soon destroyed. The effect of the light on chloride of silver was equally remarkable. Three portions, one at the distance of about an inch from the luminous ball, the other four, and the third in the focus of the violet rays, concentrated by a powerful lens, were exposed to the influence of the light for twelve minutes, and were then found to be completely discoloured; that nearest the light being almost black, the other two having a brownish black hue. Having now described the instruments contrived to facilitate the observation of the distant stations of the survey, it may not perhaps be uninteresting to add an account of their successful application to a case of considerable importance and difficulty, that occurred at the end of last season.

Slieve Snaght, the highest hill of Innishowen, about 2100 feet above the sea, and 15 miles N. of Londonderry, forms an important point in the triangulation, which connects the North of Ireland with the western islands of Scotland.

On the 23d of August last a conspicuous object was placed on its summit, that it might be observed from the Divvis Hill, near Belfast, where we were then encamped. Having continued, however, till the 26th October, enveloped in a haze so impenetrable as to render unavailing every effort made for this purpose, Colonel COLBY resolved, although the season was far advanced and the weather unsettled, that an attempt should be made to surmount this formidable obstacle, by the aid of the instruments now described. By his directions I proceeded with a party of men to the hill in question, which we reached on the 27th October. For the first ten or twelve days after our arrival we had to struggle against most tempestuous weather, and being only provided with round tents, which resisted but for a short time the violence of the successive gales, it was with some difficulty and exertion that we maintained ourselves on the hill, and preserved the instruments themselves from destruction. At length, however, the weather becoming moderate, both instruments were brought into use, and we had the satisfaction of learning from Lieutenants HENDERSON and MURPHY, to whom Colonel COLBY had confided the task of completing the observations, and whose vigilance the first glimpse of the light did not escape, that on the 9th and 10th of November both instruments had been brilliantly visible, and the observations in consequence brought to a satisfactory conclusion. The distance between the stations is 66¼ miles; but the difficulty experienced in observing Slieve Snaght appears to have arisen not so much from the distance, as from the direction passing over a range of other hills, and not far above their tops. Of twelve observations, made during the day, on the ninth and tenth of November, not more than one could have been effected without the aid of the heliostat, and many were obtained when the outline of the hill itself had ceased to be visible. The light at night was referred to a 15 inch parabolic reflector, illuminated by an argand lamp, and placed on the church tower of Randalstown, nearly in the same direction. The light on the distant hill was not only seen with the naked eye, but appeared much brighter and larger than that at Randalstown, their relative distances being nearly 67 and 15 miles. Colonel COLBY proposes employing this light to effect the observation of Ben Lomond from Knock Layd, in the north-east extremity of Ireland, a distance of not less than 95 miles; and availing himself of the position of Ben Lomond, which commands a view of the Observatory on the Calton Hill of Edinburgh, to determine by simultaneous observations, the difference of longitude between it and Knock Layd, which is nearly in the meridian of the Dublin Observatory.

It may perhaps be asked, if this mode of producing light does not admit of being applied to other purposes, besides those of a Trignometrical Survey: and, that, which naturally suggests itself, as a very extensive and important application, is the illumination of light-houses. Not being subject to explosion, it might be employed without incurring the slightest danger: and the apparatus, when fixed and permanent, might be rendered so simple, that the most ordinary care and attention would be sufficient for its management. Even on the survey this requires no peculiar address, notwithstanding the inconveniences of very exposed situations, and the necessity of rendering every article as portable as possible. The expense may appear, at first sight, the strongest objection to its general use; but, supposing rectified spirits to be employed, with the duty remitted, and no further use to be made of the oxide of manganese, after the abstraction of the oxygen,* it may be stated, in general terms, that while the intensity of light varies between 60 and 90 times that of an argand lamp, the expense would not exceed ten times. Applied to a revolving light, where four sides are illuminated, each with four reflectors, one reflector, with the lime light, might be substituted on each side, and with an increased expense of 2½ times that of the oil an intensity varying between 15 and 22 would be obtained. It will also be recollected that the oil consumed is far from constituting the most considerable portion of the expense of maintaining a light-house, and that it would probably not be underrated if estimated on an average at one-fourth. The other sources of expenditure would, in both cases, be the same. How far this increased expense would be an objection to its general introduction into light-houses, is for others to determine; but there are, I should apprehend, situations, such for example, as the Lizard Point, the Scilly Islands, or the great Skellig,† off the south-west extremity of Ireland, where the advantages of so powerful a light would not be considered too dearly purchased at such a price. Whether it would be most advantageous to collect the light with common parabolic reflectors, or to adopt the system of lenses and plane reflectors, proposed by M. FRESNEL, with such modifications as the peculiar nature of the light might admit or require, would readily be determined by experiment. There are good grounds for believing that this latter mode would be found the most economical and effective; and that a light-house might be brilliantly illuminated by a single ball of lime; in which case the expense would not exceed, if it equalled that of oil. It might not be uninteresting at the same time, to enquire what degree of light could be obtained from lime, by employing atmospheric air with common oil or tallow, as the source of heat. From some rough experiments made with this view, it seemed not improbable that it would possess considerable superiority over that of an argand lamp. But whatever might be the result of these enquiries, it will probably be admitted that enough has already been practically carried into effect, to show that no formidable difficulty stands in the way of the application of this mode of illumination to light-houses, and that the subject is not undeserving the attention of those, to whose charge these important establishments are committed.

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* It is not improbable that some simple process might be found for causing the manganese to absorb from the atmosphere the oxygen which it had lost by exposure to heat; in this case the expense would be diminished about two-thirds.

† A light-house is now building on this remarkable rock, which will be of great importance, being the first made by vessels from America.

XXIV. *On the production and formation of pearls.* *By Sir* EVERARD HOME, *Bart. V. P. R. S.*

Read May 11, 1826.

FOR the last four years I have been engaged, with the aid of Mr. BAUER's microscopical observations, in examining the mode of generation of the fresh water muscle and oyster; illustrating the changes that occur by Mr. BAUER's drawings in the same manner as has been done with respect to the egg of the pullet, the frog, the earth worm and barnacle. But as it will require another season to complete this investigation, I am desirous of preceding it with an account of the formation of pearls; the discovery of which was made in the early parts of my enquiry into the mode of generation of the fresh water muscle.

I am the more desirous of laying before the Society these new facts respecting pearls at this moment, when the public mind is directed to the improvement of the pearl fishery, and therefore every thing respecting them will be received with greater interest than at any other time.

In examining the organs of generation of the large fresh water muscle, I very frequently met with what are called seed pearls; and these were always found in the ovarium, or connected with the shell on which the ovarium lay. I at the same time accidentally discovered that all oriental pearls that are split into two halves have a brilliant cell in the centre;

this, however, where the pearl has been bored is destroyed, and upon comparing the size of the central cell with that of one of the ova, it is exactly large enough to contain it. The ova themselves are formed upon pedicles, in the same manner as the yelks of the pullet's eggs; and must, when completely formed, have a similar mode of being discharged.

From these facts I have been led to conclude, that a pearl is formed upon the external surface of an ovum; which having been blighted, does not pass with the others into the oviduct, but remains attached to its pedicle in the ovarium, and in the following season receives a coat of nacre at the same time that the internal surface of the shell receives its annual supply.

This conclusion is verified by some pearls being spherical, others having a pyramidal form, from the pedicle having received a coat of nacre as well as the ovum.

The best mode of rendering the central cell conspicuous, is setting one half of a split pearl in a ring with the divided surface outwards, and looking at the cell through a magnifying glass.

Mr. Banks, the optician, in the Strand, has succeeded in contriving an apparatus for this purpose, from which the annexed magnified drawing is taken.

It is the nacral shining lining of the central cell that produces the lustre peculiar to the pearl, which cannot be given to artificial ones.

Pearls being composed of concentric layers of nacre which are annual, must be of slow growth, and those of large size can only be found in full grown oysters.

I have engaged an intelligent person, who is on board one of the ships employed in the pearl fishery, furnished with a diving bell, to attend to every thing connected with the present enquiry; and also to go down in the bell whenever it is used, paying attention to the objects that are seen, and noting the latitude and longitude, so that he may make the first beginning of a sketch of a map of the land under the sea.

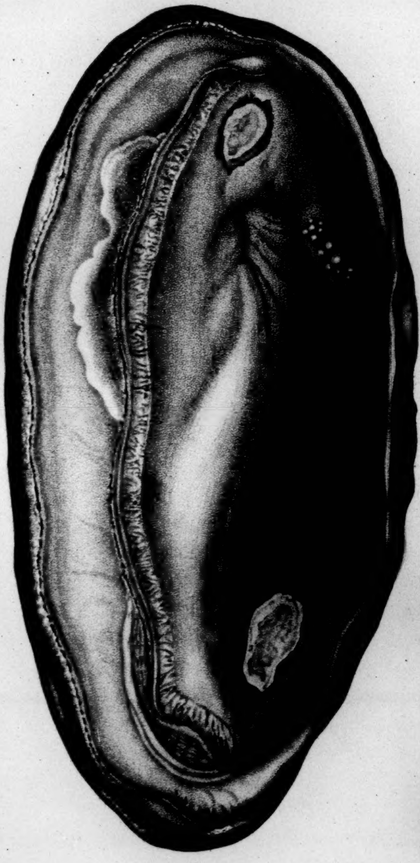
Upon stating to my friend, Mr. Children, of the British Museum, some of the above facts, he referred me to the ninth volume of the Philosophical Transactions for the following remarks, which accord in every thing with what I have stated.

Extract of a Letter from Hamburgh, by the learned Christophorus Sandius, *concerning the origin of pearls, Dec.* 1, 1673. *Translated into English.*

"The pearl shells in Norway do breed in sweet waters; their shells are like muscles, but larger. The fish is like an oyster, and it produceth a great cluster of eggs like those of crawfishes, some white, some black (which latter yet will become white, the outer black being taken off): these eggs when ripe are cast out; and being cast out, they grow and become like those that cast them: but sometimes it happens that one or two of these eggs stick fast to the sides of the matrix, and are not voided with the rest. These are fed by the oyster against her will, and they do grow according to the length of time into pearls of different bignesses, and imprint a mark both on the fish and the shell, by the situation conform to its figure."

In a second letter, Feb. 27th, 1674, in answer to one from

Fig. 1.



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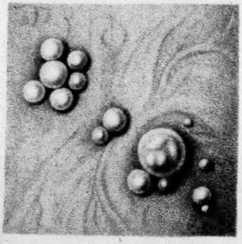
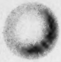


3.

4.



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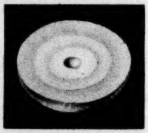

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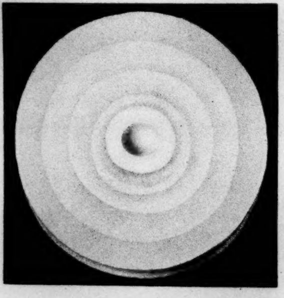

Franz Bauer del.

J. Basire sc.

the publisher, desiring to know the ground on which the author had made that assertion.

"As to the authority I have to assert such an origin of pearls, I here declare that a certain Dane, called Henricus Arnoldi, an ingenious and veracious person, having by his own experience found it so in Christiana, in Norway, with great seriousness assured me of the truth thereof."

PLATE XIII.

No. 1. A fresh water muscle, one shell removed, of the natural size.

The lining, with its attachment to the shell on which it lies, also the loose fringe upon its edge.

A portion of the foot exposed between the linings of the two shells.

A portion of the oviduct also exposed.

The lining detached from the shell removed.

The two muscles cut through which united the shells.

A cluster of small pearls lying on the outer surface of the ovarium, close to the liver.

No. 2. The above pearls magnified three diameters, firmly attached to the membrane on which they lie.

No. 3. A pearl bursting the bag in which the ovum was formed; five diameters.

No. 4. A pearl escaped from the bag; five diameters.

No. 5. Section of No. 4; ten diameters.

No. 6. Section of oriental pearl; ten diameters.

XXV. *On burrowing and boring marine animals. By* EDWARD OSLER, *Esq. Communicated February* 15, 1826, *by* L. W. DYLLWIN, *Esq. F. R. S.*

Read May 25, 1826.

THE neighbourhood of Swansea offers remarkable facilities for observing the habits of the burrowing marine animals. Many Spatangi, innumerable Annelides, and a variety of bivalves are found on its extensive sandy shores; considerable beds of decayed wood are inhabited by Pholas candida; and the rocks at the western extremity of the bay abound with Lithophagi.

My chief object in this paper, is to explain the mechanism by which the boring and burrowing shell fish form their habitations; but as there are facts connected with the burrowing of other marine animals which are yet but imperfectly understood, I shall first advert briefly to the latter.

The Nereides found in sand bury themselves by the rapid undulating motion which they employ in swimming; and they travel through the sand with great facility by extending the anterior rings, and bringing up the posterior part of the body after them. Their progress is very much assisted by the action of their numerous bristly feet.

The Arenicola piscatorum of LAMARCK (Lumbricus marinus of LINNÆUS), connects the naked with the sedentary Annelides. A viscid secretion exudes from the anterior half of the animal to which the sand adheres; and hence, when the worm is dug up, this part is generally found to be covered with an imperfect arenaceous tube; within which the motions of the body are performed with perfect freedom, but which cannot be removed entire. This tube is in fact left behind in the progress of the animal, and forms a complete lining for the hole, which it supports, and keeps pervious through its whole length. The worm is thus enabled to ascend readily to the surface, and the water is admitted freely to the branchiæ.

A few of the anterior rings of the Arenicola progressively diminish in size, and each admits of being completely folded within the ring immediately behind it. Hence, when they are fully retracted, the anterior extremity of the animal is truncated abruptly; while it is extended to a regular cone when they are projected to a point just short of developing the mouth. This is its boring apparatus. The rings being retracted, the flat head of the Arenicola is directed against the sand; when the projection of the cone opens a passage for the body. The opening thus made would however be insufficient, and the branchiæ might be injured while forced through a narrow passage; but that the animal, immediately on completing the penetration, distends the penetrating rings very considerably. By the advance of the body the opening made for it is occupied; and the rings being thus received into each other, the cone is ready to be again projected. In the progress of the animal, the gluten which exudes from it cements the surrounding sand, and gives that support to the sides of the hole, without which it would immediately fall in.

The habits of the Terebella conchilega are so remarkable, and at the same time are so easily observed, that it is scarcely possible to read the minutely accurate description of it given by PALLAS, in his Miscellania Zoologica, without a feeling of surprise at his having overlooked them. It does not indeed appear that he had ever met with the animal in its natural situation, or that he was even aware of its propensity to burrow. His observations were made upon specimens thrown on shore by storms; and that which his figure represents is evidently a starved one.

But notwithstanding these disadvantages, his description is remarkably accurate and complete. Perhaps the surface which he has denominated "ventral," might with more propriety be considered as the "*dorsal;*" but this is too unimportant to be made a subject of dispute. The tentaculæ are much more extensile than he has stated; but this he could have known only by seeing them in action. If, when observing the motions of the animal, he had put some sand at the bottom of the vessel, an experiment, his neglect of which is the more extraordinary, since he employed it in his observations on the Pectinaria belgica, he would probably have completed his subject. His specimens must have been very weak and sickly indeed if they did not at least attempt to form a tube, and to bury themselves; for I have ascertained that a serious wound, and even the loss of the entire tail, does not prevent them from attempting, and even completing these tasks.

On these shores the Terebella conchilega is the most abundant of all the larger Annelides, with the exception of the Arenicola, and perhaps of the Sabellaria alveolata. They are most numerous about midway between high and low water mark, where the sand is mixed with a large proportion of stones. By the shifting of the sands in storms, they are often buried to a considerable depth, and at other times have several inches of their tubes exposed. In the first case they speedily work up to the surface; in the other, the exposed portion of tube is soon broken off by the waves, or crumbles when dried by the sun. Their tubes are very long, and the animal remains towards the bottom, except when seeking its food. Hence specimens are to be obtained in general only in favourable situations, as it is almost impossible to dig up the whole tube.

As soon as the Terebella conchilega is removed from its tube, it throws itself into spiral folds. This contraction, which appears to be involuntary, is effected by the muscular bands, which extend the whole length of the ventral surface. The dorsal surface therefore constantly forms the outer or convex part of all the folds. If now it is placed under water upon sand, it straightens the body, and presently all the tentaculæ are seen in full action. They are extended in every direction, often to a length exceeding an inch and half; and they are seen dragging in small fragments of shells, and the larger particles of sand. These it places behind the scales projecting from the anterior and lower part of the head, where they are immediately cemented by a gluten which exudes from the animal; bending the head from side to side, while it continues to apply the materials of its tube, the conchilega has very soon formed a complete collar, which it employs itself to lengthen regularly from every part of its circumference with a persevering activity highly interesting. To fix the different fragments compactly, it presses them into their places with the erected scales, at the same time retracting the body a little. Hence, the fragments being raised by the scales, are generally fixed by their posterior edges; and thus overlaying each other, often give the tube an imbricated appearance.

When the tube is from half an inch to an inch in length, the animal proceeds to burrow; directing its head against the sand, it penetrates by a slight extension of it, effected by the circular contraction of some of the posterior rings, which presses the viscera forward. The penetrating force is very inconsiderable, because the animal having to form its tube as it descends, can advance but slowly. It is now of course obliged to avail itself of the materials with which it may happen to come into contact. Its progress is marked by the gradual disappearance of the tail, which continues to advance into the tube till the whole has entered. Soon after the conchilega is found to have turned within the tube, and its head appears at the surface. This motion is clearly effected by the longitudinal muscles (fig. 1, *c. c. c.*), which have been stated to throw the animal into folds, and which must also raise the tail to the surface whenever the fæces are to be discharged.

In summer the whole task is completed in four or five hours; but in cold weather, when the animals are more sluggish, and the gluten appears to be secreted in smaller quantity, their progress is slower in proportion.

The extremity of the tube is generally ornamented with a number of branches, composed of single rows of sandy or shelly particles. These are formed accidentally from time to time, by the adhesion of sand to the tentaculæ when thrown out in quest of prey. The gluten exuding from the tentaculæ cements the grains to each other and to the tube. I have very often known these branches to be formed by the animals I kept in confinement.

The Spatangus buries itself in the sand by the action of its bristles: those on its under surface are short and thick, diverging from the centre towards the circumference; and at their extremities becoming expanded, flattened and curved, with the convexity downward. Around the sides they are straight, longer, but much slighter than the others, diminishing in thickness towards their extremities; and, in the state of inaction, resting flat upon the body, with their points directed backward. A few long stout spines arise in a crescent from the back, which converge till their points meet. All are attached to the crest individually by a small muscular papilla.

The flat bristles have a limited rotatory motion, and admit also of being moderately depressed: the lateral ones are extended nearly to a right angle with the body, and are directed downward, even beyond the perpendicular: the long dorsal spines admit only of being erected to the perpendicular, or reclined backward to a moderately acute angle with the body.

The operations of the animal are best observed by placing it on the sand immediately after it has been dug up, as it soon perishes out of the water The motion of the short flat bristles forms in a little time, a quicksand under it, into which it sinks by its own weight so far as to enable the lowest of the lateral bristles to act upon the sand. These are directed downward, raised, and then returned to the side, to be again depressed and elevated. Thus they co-operate with the flat bristles in loosening the sand, while they throw up that which has been reduced to a semi-fluid state; and, at the same time, act in some degree as levers to depress the Spatangus with a force greater than it could have derived from its weight alone. As the animal sinks, more of the bristles are enabled to act, and its progress becomes more rapid; while the semi-fluid sand which it throws up flows slowly back upon it and covers it, when it has sunk below the level of the surface. The long dorsal spines prevent the sand from closing completely, and preserve a small round hole, by which water is admitted to the animal, and which marks its situation.

The burrowing of Bivalves into sand may be observed at all times by persons near the coast: all that is necessary is to place a few of them on a bed of sand, covered with sea-water. The animals, which from their small size or the little depth at which they reside, are particularly liable to be exposed, will generally burrow readily; but the large species will scarcely attempt to bury themselves except in the very young state. It may be proper to observe, that those thrown on shore by storms will very rarely attempt to burrow; and that there is a considerable difference in this respect, even in the animals just taken from their natural situations; some burying themselves immediately, while the greater number will remain inactive for two or three days.

When the animal would burrow, it projects and elongates the foot, distending it until every part of it, except the point, appears semi-transparent. Directing its point downward, it insinuates it into the sand until it is nearly buried. A circular motion is now given to the shell, by which its anterior point is quickly brought nearly into contact with the foot, and immediately returned to its former situation. It thus moves on the foot, as on a fulcrum, with a see-saw motion. The foot, which had been partially retracted, is again gradually projected as far as possible into the sand, when the circular motion of the shell is repeated. When the animal is moderately active, the strokes follow each other at intervals of 20 or 30 seconds. The apparent progress is at first but small; the shell, which is raised on its edge at the middle of the stroke, falling back on its side at the end of it: but when the shell is buried so far as to be supported on its edge, it advances more rapidly, sinking visibly at every stroke, till nothing but the extremity of the syphon can be perceived above the sand.

The instinct which directs the animal thus to procure a shelter, operates at the earliest period of its existence. On examining a Mya truncata, dug up on the preceding day, and which, when grown, will not attempt to burrow, I found two young ones, entangled in the folds of cuticle at the extremity of the syphon, scarcely more than a line in length, and apparently but just excluded. Being placed on sand in a glass of sea-water, they buried themselves immediately.

The motions by which the animals burrow are effected by two pair of muscles, which arise from the shell, and are inserted into the foot. The anterior muscles, (fig. 2, *a)* arise at the upper margin of the anterior adductor, and uniting immediately below the mouth, pass down together to their insertion. The posterior (fig. 2, *b*), take their origin at the upper margin of the posterior adductor; and passing forward and downward, meet near or upon the body of the animal, and advance together along its posterior edge. The foot itself is embraced by fibres given off from all these muscles.

A tube opens just within the mouth, going off nearly at a right angle with the œsophagus: it proceeds in a curvilinear direction through the body, immediately above the foot: its course is indicated in the figure by the dotted line at *c*. It is this tube which conveys the water by which the foot is distended. In some of the largest species, as Cyprina islandica, a transverse section of the foot shows a single chain of pores along its whole length, which communicate with the principal tube, and transmit the water to the cellular portion of the foot.

It is scarcely necessary to detail the action of these several organs. The point of the foot being solid, is adapted to precede the distended portion in penetrating the sand. A viscid matter is secreted from the surface of the foot when distended, which, by agglutinating the sand around it, fixes it more firmly, and thus augments the force of the stroke. The operation of the anterior and posterior muscles of the foot cannot require explanation.

The locomotive Bivalves travel by a series of motions precisely similar to those employed in burrowing, and which are effected by a similar arrangement of muscles. The foot, instead of being pointed downward, is projected horizontally just beneath the surface of the sand; and the shell is brought forward by the successive contraction of the anterior and the posterior muscles. The Venus gallina is particularly disposed thus to travel, and indeed is generally to be found most readily by the furrow which it has ploughed up in its progress. In the summer it is met with on these shores in considerable numbers, above a furlong from low-water mark; though at other seasons it is rarely seen, excepting after storms. Anodonta cygnæa offers an example of the same kind. It is thus, also, that Cyclas cornea crawls over a smooth surface, fixing itself by a viscid gluten, which exudes from the elongated foot in such quantity, that the animal in its progress leaves a silvery track like that of a snail. I have repeatedly seen small ones crawl up the side of a glass, and even support themselves on the surface of the water by a broad filament of this gluten extended from the side of the vessel.

Although not habitually residing under the sand, the Buccinum undatum is entitled to a place among the burrowing marine animals. It is often found to have completely buried itself, apparently, for shelter; and there are very strong reasons for suspecting that it habitually burrows in pursuit of its prey.

As in the Bivalves inhabiting sand, its foot is the instrument of penetration; and like them, it has the power of distending this organ to a size, nearly, if not quite equal to that of the shell. A section of the foot shows it to be divided into two nearly equal parts; the powerful muscle, which extends from the operculum to the spire, forming the upper, or posterior half, and a cellular spongy mass constituting the remainder. The lower surface of this portion is the disc on which the animal crawls; and being considerably longer than the muscle, it is folded upon itself, when retracted within the shell; and the operculum lies flat above it when it is projected and extended. A transverse section of the foot near the part where it joins the body, shows four considerable tubes penetrating the spongy portion, and very near each other; three of which are in a line parallel to, and almost in contact with the muscle; the fourth, a little below the middle one of the three. By a series of transverse sections of the foot, parallel to the operculum, we are enabled to trace these tubes; and to ascertain that they become rapidly smaller as they advance, until they are quite lost; the longest of them not admitting of being traced quite to the operculum. All these tubes are given off at the extreme anterior point of the thorax from a considerable one, (fig. 3, *l*,) which, being situated under the muscular floor of this cavity, takes a direction to the right side, and running just within the origins of the muscles of the trunk (*k*) passes out of the thorax, nearly in contact with, and on the right side of the œsophagus. It terminates nearly midway between the heart and the rectum, (*m*) opening into a considerable cavity, which has the liver underneath, and the membrane enveloping the spire above it. When the animal contracts the distended foot, the water is seen to flow out between the mantle and the shell on the right side. The tube and cavity are easily inflated by a blow-pipe introduced into one of the tubes of the foot.

The moderately distended foot can scarcely be retracted within the margin of the shell; and when fully injected, it is elastic, and of a very large size. The cavity which it opens into the sand is therefore fully adequate to receive the shell, which is drawn down into it by the contraction of the muscle of the spire. From the attachment of this muscle, the spire is the part more directly acted upon, and which is depressed in the greatest degree. Hence the notch is always uppermost, and the Buccinum, when completely buried, is enabled to communicate with the water by its respiratory syphon.

The Pholas can be observed to bore only in the young state. They are found completely buried, when so minute as to be almost invisible; and the rapidity of their growth for the first few weeks, compels them to exert themselves perseveringly in effecting the enlargement of their habitation. Hence if a few of the young animals, which may be procured in abundance very early in the spring, are placed in a pan of water upon some of the substance which they had inhabited, they will, in a short time, begin to work, and continue to do so, at intervals, for two or three days; thus affording ample opportunity for observing the process.

The only species of Pholas now found on these shores, and consequently, the only one whose habits I have had an opportunity of observing, is P. candida. It is, therefore, to this that my observations must be considered to apply more directly.

The Pholas differs in many particulars from all other Bivalves; and its peculiarities of structure are so essentially connected with its method of boring, that a full description of them is necessary.

From the syphonal, or posterior extremity of the valves, their dorsal edges are directed inward, till they meet at the hinge, whence they again separate; and, finally, converge towards their points. There is, consequently, a triangular gape behind the hinge, which is lost by bringing the dorsal edges into contact; and an egg-shaped one before it, which becomes an oval, when the anterior points of the valves are approximated. The ventral margins have also a considerable convexity. At a fourth of the length of the valves from their anterior points, their edges are rolled in to form an umbo, which is covered with a reflected semi-pearlacious fold; and at the inner part of this is a round, prominent, cartilaginous spot, the point of union between the valves. In P. candida, a short process is given off from beneath this spot in the left valve, (fig. 4, *a*) and a thin, flat ligament from a corresponding tubercle in the right, which, passing obliquely backward, meet at the middle of their length, and are thence united to their extremities. The connection thus obtained is so weak, that it is extremely difficult to remove the fish without separating the valves: it is, indeed, only strong enough to make the hinge the constant centre of motion.

A long, thin, curved process is given off from beneath the umbo, (fig. 4, *b*). Its convexity corresponds with that of the valve, and its direction is a little obliquely backward. I believe this process to be characteristic of an animal which bores mechanically by employing its shell as a rasp. The anterior and lower portion of the shell is thicker than any other part, and its surface is armed with much stronger spines; and it is this portion which the Pholas employs in boring.

The Pholas is quite destitute of the elastic ligament which connects and expands the shell in other Bivalves. Its connections are chiefly muscular, and its motions are effected exclusively by the action of muscles. It is evident that, by this arrangement, the greatest possible freedom of motion is secured.

The Pholas is provided with four sets of muscles, in addition to those belonging to the syphon. The posterior adductor, (fig. 4, *c*,) is a flat muscle, connecting the valves nearly midway between the hinge and the extremity of the shell, and having its attachments at about $\frac{1}{8}$ of an inch within its dorsal margins. It lies so superficially, that its action is seen when the animal employs it in boring. The anterior adductor (*a*) is attached to the reflected folds which cover the umbo. It extends from a point a little before the hinge, nearly to the anterior extremity of the shell, and is covered by the accessory valve. The ventral margins are connected by muscular fibres, from the opening in the mantle through which the foot is projected, as far as the origin of the syphon. A pair of muscles, which may be termed " lateral," arise from the points of the long, hooked processes, which, becoming fan-shaped, as they pass over the body, are inserted into the sides of the foot.

The offices of these muscles are peculiar. The shell is closed, not by the adductors, but by the fibres which connect the ventral margins of the valves; and it is opened by that part of the anterior adductor which lies nearest to the hinge, and which thus performs an office analogous to that of the ligament in other Bivalves. The other portion of this muscle antagonizes the posterior adductor. By its contraction, the anterior points of the valves are brought into contact, and their dorsal margins separated as widely as possible. The action of the posterior adductor reverses this state; and, in uniting the dorsal margins, expands the anterior and armed portion of the shell. The foot of the Pholas, like that of the Gasteropodes, is a flat disc, by which the animal can attach itself firmly. When it is thus fixed, the lateral muscles, acting in an oblique direction, will raise the posterior end of the shell, and press its armed extremity forward and downward: or, if one of them should contract more strongly than the other, it will bring down the corresponding side of the shell, which will be restored to its erect position by the action of the opposite muscle.

The Pholas has two methods of boring. In the first, it fixes itself by the foot, and raises itself almost perpendicularly, thus pressing the operative part of the shell upon the substance to which it adheres; it now proceeds to execute a succession of partial rotatory motions, effected by the alternate contraction of the lateral muscles, employing one valve only, by turning on its side, and immediately regaining the erect position. I have observed that this method is almost exclusively employed by the very young animals; and it certainly is particularly adapted for penetrating in a direction nearly perpendicular; so that they may be completely buried in the shortest possible time. It may be observed, that the posterior extremities of the valves are much less produced in the very minute Pholades than they afterwards become; and thus the time required to complete a habitation is still farther diminished.

But when the Pholades have exceeded two, or, at the utmost, three lines in length, I have never observed them to work in the manner I have described: the altered figure of the shell, and the increased weight of that part of the animal behind the hinge, would prevent it from raising itself so perpendicularly as at first, independent of the narrow space which it now occupies. In the motions required to enlarge its habitation, the adductors perform a very essential part. The animal being attached by the foot, brings the anterior points of the shell into contact. The lateral muscles now contract, and raising the posterior extremity of the shell, press its operative part against the bottom of the hole; and, the moment after, the action of the posterior adductor brings the dorsal margins of the valves into contact, so that the strong rasp-like portions are suddenly separated, and scrape rapidly and forcibly over the substance on which they press. As soon as this is effected, the posterior extremity sinks, and the stroke is immediately repeated by the successive contraction of the anterior adductor, the lateral, and the posterior adductor muscles.

The particles rubbed off, and which, in a short time, completely clog the shell, are removed in a very simple manner. When the projected syphon is distended with water, the Pholas closes the orifices of the tubes, and retracts them suddenly. The water which they contained is thus ejected forcibly from the opening in the mantle; and the jet is prolonged by the gradual closure of the valves, to expel the water contained within the shell. The chamber occupied by the animal is thus completely cleansed; but, as many of the particles washed out of it will be deposited before they reach the mouth of the hole, the passage along which the Pholas projects its syphon is constantly found to be lined with a soft mud.

As a British animal, I believe the Teredo to be nearly, and probably quite extinct. In the harbours of Falmouth and Plymouth, where, some years ago, it was so abundant, it is no longer to be found. In the Royal dock-yard at Devonport, I was shown two specimens which had been taken out of a pile many years before, but they were treasured as a curiosity; and I was assured by Mr. CHURCHILL, the master shipwright, that at present they are never met with. The shipping of Swansea trade to every part of the kingdom, and we have nearly four thousand arrivals in the year, chiefly from coasting voyages; but, though very few of the vessels are coppered, we never see the Teredo.

Two causes may be assigned for this complete destruction, either of which appears sufficient to account for it. An imported shell-fish, if pelagic, may naturalize itself permanently; but where, like the Teredo, it always resides near the surface, and often in situations which are left dry during the ebb, it is necessarily affected by changes of temperature; and the animal which is naturally the inhabitant of a warm climate, will be destroyed by a severe winter. It is probable that the Teredo was formerly preserved only by occasional importations, which are now prevented by the general use of copper sheathing.

Independent of causes connected with climate, the Teredo would diminish, and become extinct for want of a nidus. Where piles had been destroyed by them, care would be taken to preserve the new ones from their ravages by a covering which the young animals cannot penetrate. Valuable timber would not be placed within their reach; and wood, neglected because useless, is generally too small to contain a large Teredo. They would therefore perish from confinement before they were full grown. The Pholas dactylus, which, but a few years since, was very numerous in Swansea bay, has been lost here from this cause. The large beds of peat-wood, into which they formally burrowed, were gradually broken up by the sea, until they became so thin that the Pholades, before they were full grown, were stopped by the gravel and perished.

Having been unable to procure the living animal, I can offer to the Society only the results of my observations on some specimens sent me from the Mediterranean.

Every peculiarity of structure upon which the boring power of the Pholas has been shown to depend, exists in an equally marked degree in the Teredo. We find the thin and obliquely placed ligament of Pholas candida, the superficial muscle connecting the valves before the hinge, the posterior adductor and the muscles passing from the lateral processes to be inserted into the foot. The muscles indeed vary in their relative size, because their size is proportioned to the force they are required to exert, which differs in the two animals. Most species of the Pholas have comparatively large shells, and burrow into substances of but a moderate degree of hardness; while the shell of the Teredo is very small, and it penetrates the hardest wood. The office of the anterior adductor being only to close the valves, its size is in proportion to the weight of the shell. It is by the contraction of the posterior adductor that the animal effects the stroke; and the force of this muscle will therefore depend upon the resistance to be overcome. Hence, in the Teredo, the anterior adductor is much smaller than in the Pholas, while the posterior adductor is far more powerful in proportion. The lateral muscles are more distinct than in Pholas candida, but they are very short. It is not probable therefore that the Teredo can bore, as the young Pholades have been described to do, by the action of these muscles alone.

The posterior adductor is attached to a large process in each valve, which, in the common Teredo is a perpendicular oval plate, resting against the dorsal margin of the valve, and formed by a prolongation and expansion of the tubercle at the hinge. About a third part of it is concealed within the shell, and the upper margin is rolled outward at its anterior end. In the Teredo palmulatus this process is nearly triangular with the apex to the hinge; and is formed by an extension of the shell itself, of which it is a mere beaklike projection. Both these processes are represented in the drawings.

If all the boring shell fish penetrated mechanically, it would be reasonable to expect that their powers should evidently be in proportion to the hardness of the bodies which they inhabit: this is found to be the case in the different species of Pholas; but the Lithophagi, which would have the greatest mechanical resistance to overcome, appear to be destitute even of the smallest mechanical force. They have nothing which in the slightest degree resembles the boring apparatus of the Pholas. On the contrary, their shell, as in the bimusculous Conchiferæ, is expanded by a powerful elastic ligament, and closed by two large round internal adductors. The valves in most of the species shut close, and the foot is not an instrument adapted for firm adhesion.

Four species of Lithophagi are found in the neighbourhood of Swansea; but as the Saxicava rugosa (Mytilus rugosus of Linnæus,) is so abundant, that it may be procured in any quantity, I shall take it as the type of the family.

The general structure of Saxicava rugosa is very similar to that of the Bivalves which burrow into sand. The number and attachments of its muscles, as well of the adductors as those connected with the foot, are exactly the same. The foot is directed nearly horizontally forward, and in its state of contraction is very small, but it admits of being distended and much elongated: I have repeatedly observed it extended in young animals to the length fully equal to that of the shell: when in this state, it is seen to contain an opaque white line or tube, which passes forward from the part where it joins the body, and terminates abruptly on the under surface near its extremity. This tube becomes black when the animal has been preserved in a saturated solution of muriate of soda. The mantle is of a greyish white colour, opaque, and remarkably thick. A byssus arises from a longitudinal slit in the posterior part of the base of the foot; and passing out with this organ through the opening in the mantle, attaches the animal to some part of its hole.

I have had an opportunity of observing the young of Saxicava rugosa, before they had begun to bury themselves. The activity which they displayed was a perfect contrast to the extreme sluggishness of the older animals. They travelled over the stone by extending and fixing the foot, and then bringing the shell forward by the action of its anterior and posterior muscles, exactly in the same manner as the locomotive Bivalves. In their progress they turned the foot in different directions almost at every step, appearing to examine the surface of the stone for a suitable spot on which to fix themselves. At length they attached the byssus, and from that time no motion could be observed in the shell. I placed one of these young animals in a valve of Mya truncata; it crawled about almost without intermission for the greater part of two days, and at length fixed itself in a crevice by the side of the ligament. During the five subsequent days it was perfectly quiescent: it perished at the expiration of that time, apparently destroyed by the putrefaction of the ligament. I have taken many hundreds of the animals from their holes, and kept them in confinement till they died, but never saw one of them attempt to change its place.

It is evident that the Saxicava does not bore like the Pholas, by a rotatory motion, for the hole is not quite round, and its smallest diameter is not equal to the depth of the Saxicava, from the hinge to the ventral margin. The animal therefore has not room to turn itself; and its attachment by a short byssus imposes an additional restraint upon it: indeed, an examination of its muscles is sufficient to show that they cannot effect a rotatory motion. The only boring motion which can be supposed to be given to the shell is that effected by the action of the muscles of the foot; that, in fact, by which a Bivalve buries itself in the sand; but this, although so well adapted for travelling over a smooth surface, or for burrowing into a substance penetrable by the foot, would be ineffectual here, because the animal being already in contact with the rock, the foot cannot be projected to form a fixed point beyond the shell. Even were it otherwise, the texture of the shell is so soft, that it could make no impression upon the stone without being itself acted on; and the effect of this would be permanent, because superficial injuries of the shell are never repaired. But nothing of this kind is met with. I have even found a Saxicava fixed between two others, which was so compressed that it was quite flat, and little more than a third of its proper thickness; yet neither of the three showed the slightest mark of friction, and the cuticle of the sides in contact was as perfect as usual.

But independent of the presumption afforded by the softness and smoothness of the shell, and by the absence of any arrangement of muscles which might employ it with effect had it been strong enough to act on hard stone, it is easy to collect facts affording ample proof that the shell cannot be the instrument of penetration. I have a specimen of calcareous stone of extraordinary hardness, containing small masses of silex, some of which project into the holes formed by Saxicava rugosa, and Venerupis irus. The lime has been smoothly cleared around the base of these projecting portions from situations with which the shell could not possibly have come into contact. In another specimen, in which the lime is mixed with a large proportion of clay, there is a small round stratum of stone entirely argillaceous, and not one-twentieth of an inch in thickness. The lime surrounding this stratum has been removed by three Saxicavæ, which have bored down upon it from different directions. The shell of one of them remains in its hole, and shows that the progress of the animal was arrested by the stratum long before its death. There is a deep groove across one valve, in which the stratum is nearly buried; and which has evidently been caused, not by friction against the stratum, but by the growth of the shell on each side of it; this is shown in fig. 6.

If additional proof is required, it is supplied by the fact that many naked animals of the softest texture form their habitations in limestone. The boring Annelides are innumerable in calcareous rocks, and are found to attack every marine shell almost as soon as it has acquired sufficient thickness to afford them a nidus. They may, indeed, be considered to perform the very important office of destroying shells when they become useless; thus preventing accumulations which, in a few ages, would fill every harbour. The thick oyster shells on these coasts are always occupied by a kind of sponge. It is a fibrous yellow pulp, filling a number of irregular cells, which open freely into each other, and eventually occupy and destroy the whole shell. The cells communicate with the surface by a number of small, round, and polished holes, each of which is occupied by a tough, and apparently inirritable tube, which adheres firmly to its circumference. These holes have been observed by every conchologist, and were supposed by Montagu to be formed by Mya bidentata. The yellow substance is not peculiar to the oyster, nor is it disease; for I have met with it in dead shells, and have found it extended to the surface through a considerable thickness of coralline. The penetration of all these naked animals leads irresistibly to the conclusion, that a shell is not essential to the boring process; and it would be inconsistent with the simplicity observable in every part of nature, to suppose that she has provided such different means for accomplishing the same end.

Whatever may be the instrument of penetration, it is situated at the lower and anterior part of the animal. Where the Saxicavæ are numerous, their holes communicate very freely; and it is common to meet with one which has attached its byssus to another. In this case, it is always found that the shell of the second has been acted on in a direction, and to an extent, which corresponds with the range of the foot of the assailant. When the animals meet laterally, they become compressed and distorted in the course of their growth, but their shells are never injured.

If the Lithophagi perforate mechanically, the shell and the foot are the only instruments which they can possibly employ. It has been shown that the shell is not adapted, or even required, for this purpose; and it must be quite unnecessary to attempt to prove that a stone cannot be rubbed away by a mere vesicle, like the distended foot; especially when it is observed that the syphon, which is so much thicker and stronger, is preserved from the effects of friction by a particular strong cuticle. To ascertain that the mechanical powers of the animal are quite unequal to make an impression upon a stone, would of itself almost justify the presumption that the task is effected by a solvent.

But the theory of a solvent does not rest on mere negative proof. It appears impossible to explain, on any other grounds, the animal's exclusive choice of calcareous matter; a choice evidently depending on its inability to penetrate stones of a different nature. I have already alluded to specimens, in which silex projects into their holes, and in which a very small stratum of insoluble stone has arrested the progress of the Saxicava. An exception may perhaps be taken to the hardness of the silex, though it certainly is not harder than the stone in which it is imbedded; but I am unable to perceive how the fact connected with the other specimen is to be explained away. It may be observed too, that in this specimen the holes, instead of being smooth, and almost polished, as usual, are uniformly rough; presenting, when examined under a glass, precisely the fretted appearance which should be produced where the soluble portion of a stone has been dissolved, and the insoluble particles are left projecting from the surface; and even stronger facts may be adduced. Specimens are at all times to be obtained in which the shell has been extensively acted on by others; while the cuticle, which had necessarily been exposed to the same agent, remains uninjured, and overhanging the breach.

In Montagu's Testacea Britannica, it is stated on the authority of Pulteney, that Venerupis irus (Donax irus of Linnæus) has been found in clay; and the author affirms that he himself possessed specimens of Mya pholadia in stones not calcareous. Facts presenting so serious an exception to the general habits of the animal to bore into lime, would be fatal to the theory of a solvent, did they not admit of full explanation. The young animals, in seeking a convenient spot, almost invariably fix themselves in holes or crevices which afford them immediate shelter. Hence, they are occasionally found lodged among the roots of fuci; and they will sometimes find a shelter in stone, upon which they are unable to act. I have met once with a Mya distorta, and repeatedly with Saxicava rugosa and præcisa, lodged in soft argillaceous stone; but in every such case the hole had clearly been made by a pholas, the remains of whose shells were generally found in it more or less dissolved by the Saxicava. The specimens I observed were in masses of rock, honeycombed by the pholas; which, having probably been brought among ballast, have for many years formed part of the boundaries of the oyster banks in this neighbourhood, and which are within a very short distance of rocks abounding with Lithophagi. I have often found Mytilus edulis, and occasionally Mya truncata, in similar situations. These are the only circumstances under which I have found the Lithophagi inhabiting insoluble stones, and such an exception confirms the general rule.

I have stated, that where the Saxicavæ are abundant, their holes very often communicate; and that their shells are acted on when exposed to the foot of another. On examining a considerable number, taken indiscriminately from the same rock, I have found that the shells of more than half had been thus injured. As long as the injury is superficial, no attempt is made to repair it; but when the shell is nearly or quite penetrated, the breach becomes filled, not with new shell, but with a firm yellow substance, which is insoluble even in a strong mineral acid. It would be difficult to conceive a fact short of absolute demonstration, which could give a more decisive support to the theory of a solvent. A peculiar provision is given to the animal to preserve it from destruction by an injury, to which it is particularly exposed. The supposition of mechanical penetration would require us to believe that a newly formed substance, much softer than that which has been destroyed, can stop the progress of the mischief, and even repair it, under the continued application of the original destructive force.

I have delayed the communication of this paper for many months, from a wish to complete the subject by detecting the solvent; but every experiment I have made for this purpose has been quite unsuccessful. Litmus paper applied to every part of Saxicavæ just taken from their holes, shells stained, and sea-water tinged with the same test, in which animals of every size were kept till they died, gave no indication of the presence of an acid. The water in which a mass of rock, containing above an hundred Saxicavæ had been kept for a week, afforded a precipitate, when treated with oxalic acid, only equal to that obtained from the same quantity of common sea-water. To explain the failure of these experiments, it may be observed, that where the Lithophagi happen to be lodged in situations which afford them sufficient room and shelter, they make no attempt to enlarge their habitation. Thus, Saxicava præcisa is more frequently found among groups of Serpulæ than in a hole which it has excavated; and I have obtained full grown specimens of Hiatella arctica (Solen minutus of Linn.) attached by the byssus to a Pecten. It may therefore be presumed that the solvent is secreted only when its agency is required; and this would sufficiently explain why a free acid cannot be detected in the animal. Litmus, in the smallest quantity, acts as a poison upon them. The animals exposed to it remain with their syphons half projected, which they do not attempt to retract on being touched, and perish in a few hours. If an increased quantity of calcareous salts could be detected in water in which they had been kept, the experiment would be a decisive one; but when it is considered that the animals live in the open sea, and are to be obtained only at low tides, it is not to be expected that they will work when confined in a small vessel, and deprived of food.

Had the question been previously balanced, our inability to detect a solvent would justify strong doubts of its existence: but while all the facts connected with the natural history of the Lithophagi afford a strong and consistent support to the theory of a solvent, and are opposed as decidedly

Phil. Trans. MDCCCXXVI. Plate XIV. p.369.

Fig. 1.

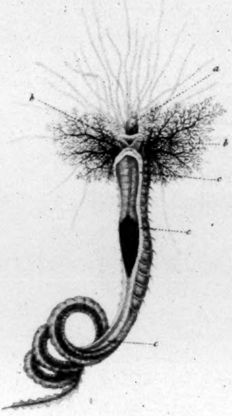

Fig. 2.

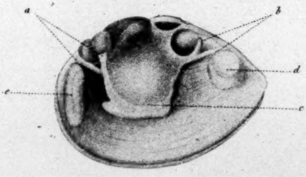

Fig. 3

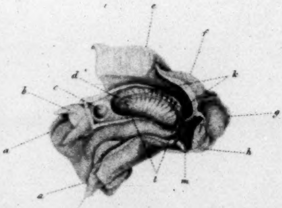

Fig. 4.

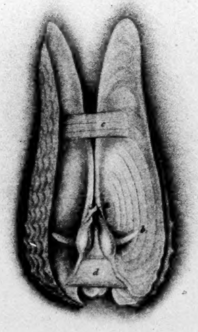

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Fig. 5.

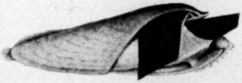


Fig. 6.

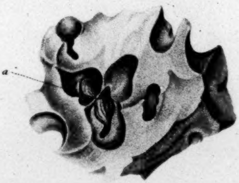


Fig. 7.

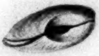


Fig. 8.

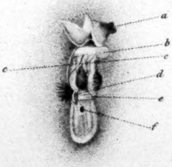


Fig. 9.

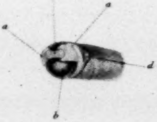
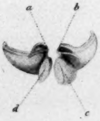


Fig. 10.



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J. Basire sc.

to the supposition of penetration by a mechanical force, the failure of the experiments cannot be considered to militate very strongly against the only inference to be drawn from the facts. I regret my inability to offer the last decisive proof, which would have set the question finally at rest; but I feel persuaded that, should future enquiries be successful in ascertaining facts which have escaped my observation, they will tend to substantiate the conclusion which I have been led to support.

EXPLANATION OF THE FIGURES PLATES XIV, XV.

Fig. 1. The Terebella conchilega, as it appears when proceeding to collect materials for its tube; *a*, the mouth; *b b*, the larger scales below the head, with which it fixes the materials of its tube; *c c c*, the course of one of the muscles by which the animal throws itself into folds. In specimens, which have been deprived of food for a few days, the muscular fibres become beautifully distinct.

The figure given by Pallas, and which is I believe the only one we possess, is not only drawn from a bad specimen, but is so very carelessly engraved, as to be inaccurate in almost every respect.

Fig. 2. A burrowing bivalve; Lutraria compressa: the left valve, syphon, mantle, and branchiæ removed.

a, The anterior, and *b*, the posterior muscles of the foot; *c*, the course of the tube by which the foot is distended with water; *d* and *e*, the adductors.

Fig. 3. The animal of Buccinum undatum; part of the

spire, and the branchiæ removed; the mantle turned to the right side; the upper part of the thorax cut away to expose its cavity, from which the boring trunk and salivary glands have been taken.

a a, The foot; *b*, the head; *c*, a kind of platform raised above the floor of the thoracic cavity, on which the point of the boring trunk rests, and which leads to the mouth; *d*, the cavity of the thorax; *e*, the mantle; *f*, the rectum; *g*, the stomach; *h*, the heart, thrown below, and to the right side of its natural situation, to allow the opening of the tube to be seen; *i*, the respiratory trunk; *k*, the origins of the muscles of the boring trunk; *l*, the course of the tube by which the foot is supplied with water; *m*, its termination.

Fig. 4. Pholas candida, one-half larger than nature. The fish has been removed, leaving only the adductors, and the shell is expanded excessively to show the hinge.

a, The ligament and process to which it is attached; *b*, the lateral process; *c*, the posterior, and *d*, the anterior adductor.

Fig. v. Pholas candida; the posterior part of the right valve removed to show the lateral muscle arising from the point of its process, and passing to its insertion into the side of the foot. Its extreme fibres extend beyond the foot on each side, and, uniting with the corresponding fibres of the opposite muscle, embrace the body. This muscle is so blended with the general covering of the body, that it cannot be seen, even with a glass, except at its origin; but its firmness makes it easy to raise it by dissection, and to display it, as in the figure, on a piece of dark paper.

Fig. vi. A mass of impure calcareous stone, containing a small argillaceous stratum, which a Saxicava rugosa has been unable to overcome.

a, The stratum confining the shell.

Fig. 7. The shell of a Saxicava rugosa which has been injured by another. The umbo has been removed, but the ligament and the cuticle, which extends from it to the anterior extremity of the shell, remain uninjured.

Fig. 8. The animal of Saxicava rugosa; the branchiæ removed, the mantle divided and thrown back; *a*, the syphon; *b*, the posterior adductor; *c c*, the posterior muscles of the foot; *d*, the foot, with its tube and byssus; *e*, the anterior adductor; *f*, the opening in the mantle for the foot. The anterior muscles of the foot are concealed by the body.

Fig. 9. Teredo palmulatus; *a a*, the boring shells; *b*, the foot; *c*, the anterior adductor; *d*, the process to which the posterior adductor is attached.

Fig. 10. Teredo communis; the boring shells separated to show the posterior adductor. *a*, The ligament; *b*, the process to which it is attached; *c*, the posterior adductor attached to one of its processes; *d*, the corresponding process in the other valve.

XXVI. *An account of some experiments relative to the passage of radiant heat through glass screens. By the Rev.* Baden Powell, *M. A. F. R. S. of Oriel College, Oxford. Communicated March* 9, 1826.

Read June 1, 1826.

(1) In a former Paper, communicated to the Royal Society, and which has been honoured with a place in the Philosophical Transactions for 1825, I attempted an investigation of the distinctive characters of two species of heating effect, in which particular reference was made to the action of transparent screens. In the present communication, my object is to examine a further point belonging to that part of the subject; and to which, as well as the former enquiry, I have been led, from considering the results obtained by M. De La Roche. The investigation given in my former paper proceeded upon the assumption, that simple radiant heat is incapable of permeating glass by direct transmission when the source is below luminosity: and the conclusion deduced from my experiments went to show, that that portion of the heat which is intercepted above luminosity, is simple heat, unaltered except in intensity, whilst that which is transmitted is of a different kind.

That this assumption, at least under all ordinary circumstances, is warranted by most decisive experiments, I conceive sufficiently certain. It appears to me, however, that in reference to its strict universality, some further enquiry is necessary. The general inference respecting transmission, deduced from DE LA ROCHE'S experiments, has, I conceive, been satisfactorily explained by mine; but there is one of his conclusions to which my principle does not apply (except in a particular case), and which might seem to afford considerable ground for the idea of an actual radiation through glass, under particular circumstances.

(2.) That distinguished experimenter found, that if radiant heat be intercepted by two transparent screens, the *additional* diminution of effect occasioned by the *second is proportionally much less* than that produced by the *first*; and the same conclusion is extended to any number of screens. This was explained by the supposition, that the heat in its passage through the first glass undergoes a certain modification, in some respects analogous to polarization, by which it is enabled to pass, with very little diminution, through the second and subsequent glasses.

(3) In those cases where the source of heat is luminous, such phænomena would receive an obvious explanation on the principle investigated in my former Paper. The simple radiant heat being stopped by the first glass, the second would produce an almost insensible diminution of the light, and therefore also of that species of heat which is transmitted in union with it.

But if the same effect is still observable below the point of luminosity, we must have recourse so some other principle of explanation. That deduced by DE LA ROCHE appears at least plausible; and though it should be considered proved, that in general heat is incapable of being radiated directly through glass, it perhaps would not necessarily follow, that it might not, under peculiar circumstances, have a power of doing so communicated to it. Though on the other hand it must be confessed that, in the present case, some difficulty would attend such a supposition.

It certainly would not be easy to conceive such a property to be communicated to the heat by the mere act of being conducted through the first glass. Again; a new property of heat is thus introduced which, it must be conceded, is not absolutely and exclusively established.

It appeared to me therefore a point of some interest to examine, in the case of non-luminous heat—in the first place, the accuracy of the fact; and secondly, if verified, whether there might not be circumstances observable in the conditions of the experiment by which it might be accounted for, without the necessity of supposing any peculiar property of heat, or a direct transmission even through the second glass.

(4.) My apparatus in following up this enquiry was similar to that described by M. DE LA ROCHE, and consisted of two tin reflectors. In one focus the bulb of a thermometer coated with Indian ink, and in the other an iron ball two inches diameter, which was heated to redness, and then cooled till it ceased to be visibly red in the dark, at which point it was placed on its stand, and a thick screen withdrawn. The indications were observed, first for the direct effect; secondly, with one glass screen interposed; and thirdly, with two. In this way I tried various experiments; in some using only one reflector; in others both; in some employing a mercurial, in others an air thermometer. I conceive it unnecessary to bring forward all the various experiments I tried, and shall content myself with giving a few of the most decisive, in a tabular form.

The first experiments I shall give, are two sets made with only one reflector, in the focus of which the bulb of a large differential thermometer was placed, and the progress of the effect traced for every 30 seconds successively in the three cases. The area of heating rays was limited by a circular aperture, about two inches diameter, in a pasteboard screen. The first screen, or that nearest the ball, was about $\frac{1}{12}$ inch in thickness, the second rather less. The divisions on the scale are arbitrary.

(5.)

One Reflector. Dist. 18 inches. No. I.

Min.	Sec.	No Screen.	1 Screen.	2 Screens.
0	—	0	0	0
	30	5	0.5	0
1	—	9	1	0.25
	30	12	1.25	0.25
2	—	13.5	1.25	0.25
	30	14.5	1.5	0.25
3	—	15	1.5	0.25
	30	15.5	1.75	0.5
4	—	15.75	1.75	0.25
	30	16	1.75	0.5
5	—	15.75	1.75	0.5
	30	15.5	1.75	0,5
6	—	15	1.75	0.25
	30	14.25	1.5	0.5
•	*		•	*
8	30		1.5	•
9	—		1.25	•
11	—			0.25

One Reflector. Dist. 18 inches. No. II.

Min.	Sec.	No Screen.	1 Screen.	2 Screens.
0	—	0	0	0
	30	5	1	0.5
1	—	9.5	1.5	1
	30	13	2	1
2	—	15	2	1
	30	16	2.25	1
3	—	16	2.5	1
	30	16.75	2.5	1
4	—	16	2.25	1.25
	30	15.75	2.25	1.25
5	—	15.5	2	1.25
	30	15	2	1.5
6	—	14.75	2	1.5
•	•			•
8	30			1.5
9	—			1.25
11	—			1.25
	30			1

(6.) In these experiments it appears that the effect with two screens is in a much greater ratio to that with one, than this to the direct effect. It is also obvious on inspection, that the progress of the direct effect follows a different law from that with one screen, and this again from that with two; and in particular, that the maximum in the last case does not occur till considerably later than in the former. But if the effects were produced by a direct transmission, though their intensity would be diminished, they would follow a similar law; and the maximum would take place at the same time. These results then tend to show that the effect takes place by a secondary radiation. But from the inaccurate nature of the instrument, I place no reliance on these results alone.

(7.) To examine the point in a more complete and satisfactory manner, the two following sets of experiments were made with particular care, in which a mercurial thermometer was used, and the temperatures of the screens observed by means of a small thermometer attached to the face of each away from the ball, towards its central part: the bulb being kept in contact by the spring of a wire with which the thermometer was fastened. These thermometers were very nearly equal in size; the diameters of their bulbs being about 0.175 inch; that of the focal thermometer 0.55 inch, and detached about an inch from the mounting. It was graduated to quarters of centigrade degrees; the others only to whole degrees. Both the screens were of plate glass: the first or nearest the source of heat about $\frac{1}{12}$ inch in thickness; the second about $\frac{1}{8}$ inch.

(8.)

2 Reflectors, 15 inches distant. **No. I.**

1st. Screen 3½ inches. 2d. 5 inches from ball.

		No Screen.	1 Screen.		2 Screens.		
Min.	Sec.	Focal Therm.	Focal Therm.	Temp. of Screen.	Focal Therm	Temp. of 1st Screen.	Temp. of 2d Screen.
0	—	16.75	16.75	17.5	16.5	18	17
	30	20	17	20	16.5	20	17
1	—	22.5	17.25	21	16.75	22	18
	30	24.5	17.5	24	17	24	18
2	—	25.75	17.75	26	17	25.5	18.5
	30	27	18.	27	17	28	18.5
3	—	28	18.	28.5	17	29.5	18.5
	30	29	18.25	29	17	30	18.5
4	—	29.25	18.25	30	17	31	18.5
	30	29.5	18.5	31	17	31.5	18.75
5	—	29.75	18.5	31	17	32	18.75
	30	30	18.5	31	17	32	18.75
6	—	30	18.5	31	17	32	19
	30	29.75	18.5	31	17	32	19
7	—	29.5	18.5	31	17	32	19
	30	29.5	18.5	31	17	32	19.5
8	—	29	18.5	31	17	31.5	19.5
	30		18.5	30.5	17	31	19.5
9	—		18.5	30	17	31	19.5
	30		18.25	30	17	31	19.5
10	—		18.25	30	17	30.5	19.5
	30		18.25	29.5	16.75	30	19.5
11	—		18.25	29	16.75	29.75	19.5
	30		18.25	29	16.75	29.5	19.5
12	—		18.25	28.5	16.75	29.	19.25
	30		18.25	28.5	16.75	28.75	19.25
13	—		18.25	28.5	16.75	28.5	19
	30		18	28	16.75	28.25	19
14	—		18	28	16.75	28	19

2 Reflectors, 12 inches distant. No. II.
1st Screen 2 in.—2nd 3½ in. from ball.

		No Screen.	1 Screen.		2 Screens.		
Min.	Sec.	Focal Therm.	Focal Therm.	Temp. of Screen.	Focal Therm.	Temp. of 1st Screen.	Temp. of 2d Screen.
0	—	17	17	18	17	17	17
	30	21	17.25	20.5	17	19.5	17
1	—	24	17.75	25	17.25	25.5	17.5
	30	26	18	27	17.5	30	18
2	—	28	18.5	30	17.5	33	18.5
	30	29.5	18.75	32.5	17.5	35	18.5
3	—	30.5	19	34	17.5	37	19
	30	31	19.25	35	17.5	38	19.5
4	—	31.5	19.5	36	17.75	39.5	20
	30	31.5	19.75	37	17.75	40	20
5		31.5	19.75	37.5	17.75	40.5	20
	30	31.5	19.75	37.5	17.75	40.5	20.5
6	—	31.25	19.75	38	17.75	40.5	20.5
	30	31.25	19.75	38	17.75	40.5	21
7	—	31	19.75	38	17.75	40	21
	30		19.75	37.5	17.75	40	21
8	—		19.75	37.5	17.75	39	21
	30		19.75	37	18	39	21
9	—		19.75	36.5	18	38.5	21
	30		19.75	36	18	38	21
10	—		19.75	35.5	17.75	37.5	21
	30		19.75	35	17.75	37	21
11	—		19.5	35	17.75	37	21
	30		19.5	34.5	17.75	36.5	21
12	—		19.5	34	17.75	36	21
	30		19.5	33.5	17.75	35.5	21
13	—		19.25	33	17.75	35	21
	30		19.25	33	17.75	34.5	21
14	—		19.25	32.5	17.75	34	21
	30				17.75	33.5	21
15	—				17.75	3	21

9.) From these results (with which all the others I obtained closely agree) I conceive it fully appears,—

1st. That the additional diminution occasioned by the second screen is proportionally much smaller than that occasioned by the first. Thus De La Roche's conclusion is shown to hold good, not only in the case of luminous, but also of non-luminous hot bodies; which is perhaps of consequence, as I believe doubts have been entertained respecting it; and it may be remarked, that here the greater thickness of the second screen would be against such a result.

2dly. If the progress of the indications of the direct effect be followed, it appears that the rise in the first 30 seconds is the greatest, and those in the subsequent periods gradually diminish.

3dly. With one screen, the effect in the first period is equal to, or even less than those in the subsequent ones; and if we follow the temperature of the first screen, it appears to sustain a rapid increase at first, and afterwards continues gradually to rise till sometime after the focal thermometer has become stationary.

The progress of the focal thermometer exactly accords with what must be the heating effect of the screen as a source; viz. rising slowly at first as the screen acquires heat sufficient to supply it, and remaining stationary so long as the still increasing temperature of the screen could balance its loss of heat.

4thly. With two screens, there is no rise till the 2d half minute; when it is not greater than in the next half; after which the thermometer becomes stationary; and this trifling effect exactly accords with what the temperature of the second screen should produce. It does not begin till the second screen has acquired a higher temperature, and it is stationary while the temperature of the screen continues to increase; and the temperature of the second screen is such as is clearly accounted for from the heating effect of the first. It does not begin to rise till after that of the first has risen: it continues stationary some time after the first has begun to cool, as the first screen did when the iron was cooling. But, as in this case, the source of heat was cooling during the whole time of the experiment, whilst in the other it was heating during the first part of the time, it follows, that a greater proportional temperature should be communicated to the second screen by the first, than to the first by the iron ball.

Other circumstances will partially co-operate in producing this effect; as the greater proximity of the second screen to the thermometer: also more heat might be lost in communicating an equable temperature to the first screen from its central and most heated part; whilst the heat would be thus more equally radiated to all parts of the second without such loss.

Thus it appears that the fact stated by M. De La Roche is fully substantiated; while on the other hand it is satisfactorily accounted for, without supposing any new property of heat, or any direct radiation through glass.

(10.) I have been more particularly led to this enquiry, from becoming acquainted with some experiments on the subject by Mr. Ritchie, published in the Edinburgh Philosophical Journal, No. XXII. p. 281; in which, among other conclusions (to which I shall presently advert), he maintains, that at high temperatures below luminosity a portion of the heat radiates through glass of ordinary thickness, but without giving any details by which it can be made to appear that the effect is not sufficiently accounted for by secondary radiation. He has also given an explanation on his own theory of the conclusion I have above examined, as deduced from DE LA ROCHE's experiments; but both that conclusion, and with it any such explanation, are now I conceive shown to be unnecessary.

(11.) The principal result obtained by Mr. RITCHIE, and described in this Paper, is however of a nature deserving more attention. It exhibits an *exception* to the general law, in the instance of *glass of extreme thinness;* in which case the heat from a source below luminosity appears capable of radiating through such a screen when *transparent*, but not when rendered *opaque*.

As experiments conducted like those of Mr. RITCHIE, with air thermometers, are always liable to uncertainty, I conceived it desirable to try the same thing with a mercurial thermometer.

(12.) I used as screens pieces of a large bulb blown to an extreme degree of tenuity; these were attached (as in Mr. RITCHIE's experiments), to a circular aperture about $\frac{3}{4}$ inch diameter in a pasteboard screen. A second screen of milled board, with an aperture of 1 inch diameter, was placed about an inch from the first; and behind this the iron ball, previously cooled to just below visible redness. The same thermometer as in the preceding experiments was suspended at about $\frac{1}{2}$ inch from the screen. I made various trials, comparing the effect of the thin transparent screen with a similar opaque one: in some instances using three thicknesses of the glass, and afterwards the middle piece blackened on both sides with the soot of a candle; and in others two thicknesses, the inner surface of one being afterwards blackened; thus forming an opaque screen with the same vitreous surface. In all the experiments a thermometer suspended by the side of the apparatus did not vary. The results of two sets of experiments are as follows:

Distance 6 inches. **No. I.**

Min.	Sec.	3 Glasses.		Middle Glass blackened.		2 Glasses. 2d blackened.
		Exp. 1.	Exp. 2.	Exp. 1.	Exp. 2.	
0	—	17.25	17.25	17.25	17.25	17.25
	30	17.5	17.5	17.5	17.5	17.5
1	—	17.75	17,75	17.5	17.75	17.75
	30	17.75	17.75	17.75	17.75	17.75
2	—	18	17.75	17.75	17.75	18
	30	18	18	17.75	17.75	18
3	—	18	18	18	18	18

Distance 3 inches. **No. II.**

Min.	Sec.	1 Glass.	2 Glasses.	2 Glasses. 2d blackened.	
				Exp. 1.	Exp. 2.
0	—	15.25	15.5	16	16
	30	16	16.25	16.5	16.75
1	—	16.5	16.5	16.75	17
	30	17	17	17.25	17.25
2	—	17.25	17.25	17.75	17.75

(13.) From these results, I think it will be admitted that no sensible difference appears in the two cases, sufficient to warrant any such conclusion as that alluded to. If the experiments described in this paper be regarded as sufficiently accurate, it follows upon the whole, that two principal exceptions to the general law, that simple heat cannot radiate through glass, are done away.

St. Helen's Place, March 9, 1826.

XXVII. *The Bakerian Lecture. On the relations of electrical and chemical changes. By Sir* Humphry Davy, *Bart. Pres. R. S.*

Read June 8, 1826.

I. *Introduction.*

A long time has elapsed since I read before this Society the Bakerian Lecture on the Chemical Agencies of Electricity. The general laws of decomposition developed in that Paper were immediately illustrated by some practical results, which the Society did me the honour to receive in a very favourable manner; and which, by offering a class of new and powerful agents, led me away for many years into a field of pure chemical enquiry: and it is only lately, and on an occasion which is well known, that I have again taken up the subject of the general principles of electro-chemical action. After a number of new experiments, which I shall have the pleasure of laying before the Society, and notwithstanding the various novel views which have been brought forward in this and in other countries, and the great activity and extension of science, it is peculiarly satisfactory to me to find that I have nothing to alter in the fundamental theory laid down in my original communication; and which, after a lapse of twenty years, has continued, as it was in the beginning, the guide and foundation of all my researches.

I am the more inclined to bring forward these new labours at the present moment, though they are far from being in a finished state, because the discovery of OERSTED and that of MORICHINI, illustrated by some late ingenious enquiries, connect the electro-chemical changes with entirely new classes of facts, and induce a hope that many of the complicated phenomena of corpuscular changes, now obscure, will ultimately be found to depend upon the same causes, and to be governed by the same laws; and that the simplicity of our scientific arrangements will increase with every advance in the true knowledge of nature.

II. *Some historical details.*

As I am not acquainted with any work in which full and accurate statements on the origin and progress of electro-chemical science are to be found, and as some very erroneous statements have been published abroad, and repeated in this country, I shall take the liberty of laying before the Society a short historical sketch on this subject; which is the more wanted, as the journal in which the early discoveries were registered has long been discontinued, and is now little known or referred to.

As there are historians of chemistry and astronomy who date the origin of these sciences from antediluvian times, so there are not wanting persons who imagine the origin of electro-chemical science before the discovery of the pile of VOLTA; and RITTER and WINTERL have been quoted* amongst other persons as having imagined, or anticipated the relation between electrical powers and chemical affinities, before the period of this great invention. But whoever will read with attention RITTER's "Evidence that the galvanic action exists in organized Nature,"* and WINTERL's "Prolusiones ad Chemiam Sæculi decimi noni," will find nothing to justify this opinion. RITTER's work contains some very ingenious and original experiments on the formation and powers of single galvanic circles; and WINTERL's some bold, though loose speculative views† upon the primary causes of chemical phenomena: and in the obscurity of the language and metaphysics of both these Gentlemen, it is difficult to say what may not be found. In the ingenious, though wild views, and often inexact experiments of RITTER, there are more hints which may be considered as applying to electro-magnetism than to electro-chemistry; and WINTERL's miraculous "*Andronia*" might, with as much propriety, be considered as a type of all the chemical substances that have been since discovered, as his view of the antagonist powers, the acid and basic, can be regarded as an anticipation of the electro-chemical theory. The queries of NEWTON at the end of his "Optics" contain more grand and speculative views that might be brought to bear upon this question than any found in the works of modern electricians;‡ but it is very unjust to the experimentalists

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* OERSTED, translated by MARCEL, 1813.

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[*] Jena, 1800.

[†] As a specimen of the Prolusiones, I shall give a few articles from the Index, which will show the character of the work. Prolusiones, pag. 256, et seq.

256. "Adamas est Andronia.
260. "Andronia cum Plumbo creat Barytam, cum Ferro Chalybem.
262. "Carbo est acidus cum Atmosphæra basica.
263. "Chromium non est nisi Calx Magnesii acida.
——. "Cuprum cum Andronià coalescit in Molybdænum.
268. "Scintilla electrica formatur à Principiis Conductorem primum et secundum animantibus, ac inter se concurrentibus; est gravis, habet effectum electricitati contrarium."

[‡] See the eloquent observations of Mr. CHENEVIX on the subject of WINTERL's Theory, Annales de Chim. Vol. 50, 2 Cap. 175.

who, by the laborious application of new instruments, have idscovered novel facts and analogies, to refer them to any such suppositions as, " that all attractions, chemical,* electrical, magnetic, and gravitative, may depend upon the same cause;" or to still looser expressions, in which different words are used and applied to the same ideas, and in which all the phenomena of nature are supposed to depend on the Dynamic system, or the equilibrium and opposition of antagonist powers.

The true origin of all that has been done in electro-chemical science was the accidental discovery of MM. Nicholson and Carlisle, of the decomposition of water by the pile of Volta, April 30, 1800.† These Gentlemen immediately added to this capital fact, the knowledge of the decomposition of certain metallic solutions, and the circumstance of the separation of alkali on the negative plates of the apparatus. Mr. Cruickshank, in pursuing their experiments, added to them many important new results, such as the decompositions of muriates of magnesia, soda and ammonia, by the pile; and that alkaline matter always appeared at the negative, and acid at the positive pole:* and Dr. HENRY about the same time made some unsuccessful attempts to decompose potassa in solution by the pile, and confirmed the general conclusions of MM. NICHOLSON, CARLISLE and CRUICKSHANK. In the month of September in this year, I published my first Paper on the subject of Galvanic Electricity, in NICHOLSON's Journal, which was followed by six others:† the last of which appeared in January, 1801. In these Papers I showed that oxygen and hydrogen were evolved from separate portions of water, though vegetable and even animal substances intervened; and conceiving that all decompositions might be polar,‡ I electrised different compounds at the different extremities, and found that sulphur and metallic substances appeared at the negative pole, and oxygen and azote at the positive pole, though the bodies furnishing them were separate from each other. In the same series of papers I established the intimate connexion between the electrical effects and the chemical changes going on in the pile, and drew the conclusion of the dependence of one upon the other. In 1802 I proved that galvanic combinations might be formed from single metals, or charcoal and different fluids chiefly acid and alkaline, and that the side or pole of the conducting substance in contact with the alkali was positive, and that in contact with the acid, negative; and in the same year I published, that when two separate portions of water, connected by moist bladder or muscular fibre, were electrised, nitromuriatic acid appeared at the positive, and fixed alkali at the negative pole.‡ In the same year Dr. WOLLASTON placed the identity of the cause of galvanism and electricity, which had been always maintained by VOLTA, out of all doubt, by some very decisive experiments.

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* In the Systême Universelle of M. Azais, not only are all the phenomena of nature referred to the same cause, but specific reasonings upon the mode of its operation given. In this work, published in 1810, not only is the identity of Magnetism and Electricity insisted on, but an attempt is made to explain the manner in which the two electrical fluids produce the magnetic phenomena, pag. 239 Vol. I. " Ainsi ces deux ordres de phénomènes sont tres ressemblans. Repetons que toutes leurs différences résultent uniquement de ce que les deux fluides sont moins intenses lorsq'ils produisent les phénomènes du Magnétisme que lorsq' ils produisent les phénomènes du Galvanisme, &c." It requires only the same principle as that censured in the text to refer to this author the discovery of Oersted and the speculations of Ampère. M. Azais, in his " fluides mineure et majeure," finds all the causes of the acid and alkaline properties of bodies:—slow combinations, the heat produced, and all the phenomena of chemical change; and his reasonings are often very ingenious.

† Nicholson's Journal, Vol. 42, page 183.

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* NICHOLSON's Journal, Vol. IV. p. 190. † Ibid. pp. 275, 326, 337, 394, 380.

‡ Journal of the Royal Institution, 1802, First Series.

In 1804, MM. HISINGER and BERZELIUS stated that neutro-saline solutions were decomposed by electricity, and the acid matter separated at the positive, and the alkaline matter at the negative poles; and they asserted that in this way muriate of lime might be decomposed; and drew the conclusion that nascent hydrogen was not, as had been generally believed, the cause of the appearance of metals from metallic solutions. These valuable observations ought to have explained distinctly the source of the appearance of acid and alkaline substances at the two extremities of the pile, yet the Paper was never translated into English, nor at all attended to; and one of their facts was contradicted by the assertion of, generally, a very accurate observer, Mr. CRUICKSHANK, who in his early experiments mentioned that he had not been able to decompose muriate of lime in the circuit.

In 1805 various statements were made, both in Italy and England, respecting the generation of muriatic acid and fixed alkali from pure water. The fact was asserted by MM. PACCHIONI and PEELE, and denied by Dr. WOLLASTON, M. BIOT, and the Galvanic Society at Paris.* Mr. SYLVESTER, who conducted his experiments with some care, stated, that if two separate portions of water were electrised out of the contact of substances containing alkaline or acid matter, acid and alkali were generated; so that up to this time the question, whether these substances were liberated from their combinations, or formed from their elements by electricity, could not be considered as decided: a circumstance not so much to be wondered at, when the novel and extraordinary nature of the whole class of galvanic phenomena is considered.

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* Some writers have very incorrectly referred the origin of these researches to the observations of HISINGER and BERZELIUS; Annales de Chem. Vol. 51. 1 Cap. pag. 167; but these observations were never quoted by any writer of the day on the pretended production of muriatic acid and alkali; and I was not acquainted with them till after my fundamental experiments were finished; and, when in drawing up an account of them, I looked back through the whole series of periodical publications to find accounts of experiments bearing upon the same question, and I believe I first directed the public attention to the value of those researches.

It was in the beginning of 1806* that I attempted the solution of this question; and after some months' labour I presented to the Society the Dissertation, to which I have referred in the beginning of this Lecture. Finding that acid and alkaline substances, even when existing in the most solid combinations, or in the smallest proportion in the hardest bodies, were elicited by Voltaic electricity, I established that they were the results of decomposition, and not of composition or generation; and referring to my experiments of 1800, and 1801 and 1802, and to a number of new facts, which showed that inflammable substances and oxygen, alkalies and acids, and oxidable and noble metals, were in electrical relations of positive and negative, I drew the conclusion, *that the combinations and decompositions by electricity were referrable to the law of electrical attractions and repulsions*, and advanced the hypothesis, " *that chemical and electrical attraction were produced by the same cause, acting in one case on particles, in the other on masses;*" *and that the same property, under*

Whoever will take the trouble to read the Bakerian Lecture for 1806, will be convinced of the gradual developement of the whole subject from the investigation respecting the pretended formation of muriatic acid and fixed alkali.

* Phil. Trans. 1807.

different modifications, was the cause of all the phenomena exhibited by different Voltaic combinations.

Believing that our philosophical systems are exceedingly imperfect, I never attached much importance to this hypothesis; but having formed it after a copious induction of facts, and having gained immediately by the application of it a number of practical results, and considering myself as much the author of it as I was of the decomposition of the alkalies, and having developed it in an elementary work, as far as the present state of chemistry seemed to allow, I have never criticised or examined the manner in which different authors have adopted or explained it,—contented, if in the hands of others it assisted the arrangements of chemistry or mineralogy, or became an instrument of discovery. And having now given what I believe to be a faithful sketch of its origin, I shall not enter into an examination of those works which have induced me to make this sketch, and which contain partial or loose statements on the subject, and which refer the origin of electro-chemistry to Germany, Sweden and France, rather than to Italy and England and which attribute some of the views of the science, which I first developed, to philosophers who have never made any claim of the kind, and who never could have made any, as their works on the subject were published many years after 1806.

III. *On the modes adopted for detecting the electrical states of bodies, and definitions of terms.*

That the statements made in the following sections may be more distinct, I shall say a few words of the mode in which the different conditions of electrical action were ascertained, and describe the manner in which I have used the terms which have been adopted in electro-chemical science.

In determining the nature of the electrical action in what may be called the closed circle, or the combinations in which, according to the language used on the Continent, electrical currents exist, I have employed instruments constructed upon the same principles as the galvanometer of Professor CUMMING, or the multiplier of Professor SCHWEIGGER. Silver wire, covered with silk, about $\frac{1}{70}$ of an inch in diameter, was folded round a small wooden frame, so as to fill a narrow deep groove: the two extreme wires were parallel, and the convolutions as nearly as possible in the same perpendicular: a small tube containing a filament of silk was passed through the centre of the convolutions of wires, to which a delicate magnetised needle was suspended; which, when the apparatus was properly disposed, rested with its north pole between the two extremities of the wires. This instrument, which contained 60 circumvolutions of wire, was found sufficiently delicate for most purposes of experiment; but in a few instances, in which very weak electricities were to be determined, I used another apparatus, in which the same kind of wire was fastened, in concentric circles, round two portions of glass tube, in such a manner that radii from the inner circle would have passed through all the wires, and in which increased mobility was given to the system by two needles exterior to it and connected with it, placed one above, the other below the central needle, with their poles in the same directions, but opposite to those of the central needle, which was so magnetised that its directive power was neutralised by the power of the other two needles.*

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* This arrangement differs from that of M. NOBILE only by a duplication of effect.

To illustrate the operation of these apparatus, I shall state, that when the lower terminating wire, which was to the left, or east of the north pole, was connected with a piece of zinc, and the upper one with a piece of platinum, both being in common water, the deviation of the central needle was eight or ten degrees, the south pole turning to the east or left hand; which may be considered as indicating that the current of electricity was from the platinum to the zinc through the wire, and that the surface of the zinc in the fluid was positive with respect to the opposite surface of platinum; and in using the terms positive and negative, I beg to be understood as applying them to the metallic surfaces in contact with the fluid.

For determining weak electricities of charge, or as it is sometimes called, of tension, I used VOLTA's condenser connected with BENNET's electrometer, and sometimes with one constructed on the principle of BEHRENS, consisting of an insulated gold leaf, or what I found better, a silk filament, made conducting by impalpable charcoal powder, to receive the charge, placed between the poles of a dry pile consisting of 400 circles of silver and gold foil, of the third of an inch in diameter, or 50 of zinc and silver of the same size, with paper intervening; the attraction of the gold leaf or the filament, either to the positive or negative pole, indicates the nature of the charge: and, as in cases of electro-chemical action there are always two corresponding opposite states, I considered the part of the system which touched the conductor as possessing the same electrical state with that exhibited by the leaf. I have never however put much dependence upon indications given by this instrument, unless they were confirmed by other results; having found them very uncertain and influenced by the state of the condenser and the atmosphere.

IV. *On the electrical and chemical effects exhibited by combinations containing single metals and one fluid.*

I know of no class of phenomena more calculated to give just views of the nature of electro-chemical action than those presented by single metals and fluids; and as their results are, with one or two exceptions, entirely new, I shall describe them with some degree of minuteness. When two pieces of the same polished copper, connected with the platinum wires of the multiplier, were introduced at the same time into the same solution of hydro-sulphuret of potassa, there was no action; but if they were introduced in succession, there was a distinct and often, if the interval of time was considerable, a violent electrical effect—the piece of metal first plunged in being negative, and the other positive.

This result depends upon the circumstance of the production of a new combination, which is negative with respect to the metal; for after the formation of the sulphuret of copper, the plate of copper that has been first plunged into the solution exhibits the same negative state with respect to polished copper, whether introduced into saline solutions, or alkaline or acid menstrua. The electrical effect therefore does not depend on so simple a condition as would at first appear, and it may be in fact referred to the combinations containing two metallic substances and one fluid.

The grey sulphuret of copper is negative, in solutions of hydro-sulphuret, to clean copper, and the superficial coating has apparently similar electrical powers to this substance.

Copper, in the state of protoxide, is negative, not only with respect to metallic copper, but likewise with respect to the sulphuret; a circumstance which explains many singular and apparently anomalous circumstances with respect to the action of hydro-sulphuret on copper. I have often found the order which I have mentioned, of metallic copper being positive with respect to copper that had been a few seconds in solution of hydro-sulphuret, reversed in a singular and capricious way; but on investigating the cause, I found that the copper was tarnished; and on heating any kind of polished copper strongly, so as to produce a thin coating of oxide any where on its surface, it became strongly negative to copper plunged in solution of hydro-sulphuret: the same effect was produced by the action of acids.

There are some singular circumstances connected with the violent and intense chemical action of copper on solutions of hydro-sulphurets, which are worthy of being described. When a piece of copper connected with the multiplier has been for a minute in strong solution of hydro-sulphuret of potassa, on introducing a piece of polished copper connected with the other wire, there is often a violent and momentary negative charge communicated to it, which sends the needle through a whole revolution: it then oscillates, and almost immediately returns, and takes the direction which indicates that the piece first plunged in is negative. This effect continues for some minutes, then becomes weaker; at last the two sides are in equilibrium, and the piece which was first plunged in now becomes positive with respect to the other. The first described of these effects seems to depend upon the discharge, by the clean copper, of the negative electricity accumulated by the contact of the plate first plunged in, before the relative states produced by the metallic contact and the regular currents occur; and the second, to the detaching or peeling off of the coat of sulphuret, which has the effect of exposing a clean surface, and which effect is probably occasioned by the oxidation of the positive side of the plate.

There are few electrical actions more intense than those produced by the operation of hydro-sulphurets on copper in these different circumstances; so much so, that I have constructed a Voltaic battery which decomposed water, by six combinations, consisting merely of thin slips of copper, of which one half had been exposed to the solution about a minute before the other half: of course, the oxidating surface was on the side of the clean or latest exposed metal.

With lead, and alloys of tin and lead and iron, there are the same phenomena, but much feebler electrical action, the metallic surface which is first introduced being the negative surface; and the principles of this kind of action are precisely the same as those of copper and hydro-sulphurets.

Zinc, platinum, and metals which have no chemical action on solutions of hydro-sulphurets, produce no phenomena of this kind; silver and palladium, which act powerfully with these menstrua, produce very decided effects; but the compounds they form in them being positive with respect to the pure metals, the phenomena are the reverse of those offered by the more oxidable metals: the surface plunged first into the solution is the positive surface, and it retains this relation in alkaline, acid, and saline solutions, presenting peculiarities dependent upon the change of surface, which I shall refer to again hereafter.

The production of electrical currents by single metals and single fluids, though most distinct in the cases I have just named, yet occurs generally whenever new substances which can adhere to the metals are produced in chemical action. Thus in acid solutions of a certain strength pieces of the same zinc, tin, iron, and copper, exhibit similar phenomena; the surface first plunged into the acid being tarnished, or retaining a slight coat of oxide, is negative to the metal plunged in afterwards, and the relation is sustained in saline or alkaline solutions. The same effect is caused by producing a coat of oxide by heat on the surface, or even by applying it artificially. The oxidated surface is negative with respect to the other.

Zinc, which dissolves in a strong solution of potassa, giving off hydrogen copiously, exhibits exactly the same phenomena in this solution; the tarnished metal, or that first introduced, being negative with respect to the other. Tin likewise in solution of potassa, having been introduced long enough to have tarnished, is strongly negative with respect to polished tin.

Even the noble metals obey the same law. Silver, that has been tarnished by the action of nitric acid, is negative to polished silver in diluted acid; and gold and platinum, that have been acted on by aqua regia, are negative in that acid to the clean metals.

The intimate connexion displayed in all these cases between the chemical and electrical phenomena, becomes still more remarkable when the nature of the changes taking places in circles of this kind is considered.

Oxygen, which may be considered as negative with respect to all the metals, and sulphur, which is negative with respect to the oxidable metals, by their combinations with metals respectively positive to them, produce compounds negative with regard to those metals. And in the chemical changes, the results are such as must ultimately restore the equilibrium, hydrogen or sulphuretted hydrogen passing to the negative side, and oxygen to the positive side; so that the oxides are revived; and not only is the equilibrium restored, but the poles sometimes changed. Thus tin that has tarnished in acid, remains for some time negative in solution of alkali, but gradually as the oxide upon it is revived by the hydrogen determined to this surface, it loses its negative power; and the other surface, now tarnished by the action of the alkali, gains this power, whilst the opposite surface becomes positive.

V. *Of electrical combinations, consisting of two imperfect, and one perfect conductor; or two fluids and a metal, or charcoal.*

To understand clearly the nature of the action in this kind of electrical combination, it is necessary to consider the nature of imperfect conducting bodies, water, or saline solutions. These bodies may be regarded as having the same relations to electricities of very low intensity, that elastic fluids have to the electricities of glass, sealing wax, or the common machine. They communicate the electrical polarities of the metals, but do not appear capable of receiving such polarities, or at least of retaining them; and the electrical equilibrium, when broken in them, seems to be rapidly restored by a new arrangement or attraction of certain of their elements. For instance, if we introduce the positive and negative poles from a very powerful Voltaic battery into the extremities of a basin filled with solution of muriate of lime, and place in the circuit different wires of platinum, every wire will possess a positive and negative pole, and there will be no division of the fluid into two parts, one positive, the other negative; and when the two wires are withdrawn, they alone having been used, the electrical appearances immediately cease; and metallic wires unconnected with the battery made to occupy their places, exhibit no electrical phenomena: and in all experiments of this kind, the well known phenomena of the developement of chlorine and oxygen and acid matter at the positive, and hydrogen, alkaline matter, &c. at the negative pole, takes place.

Acid and alkaline matters, when perfectly dry and non-conducting, become on contact negative and positive; as I have shown is the case with oxalic acid and lime; but this effect is similar to that of glass and silk, and the results is a common electricity of tension. And when acids and alkalies combine, their union being apparently the result of the same attractive powers acting on the particles which would produce their electrical relations as masses, they exhibit no phenomena of electro-motion; and such phenomena, when they occur in combinations in which acids and alkalies unite, always depend upon the contact of the metal with the acid and alkaline matter, change of temperature, evaporation, &c. and never on the combination of the acid and alkali.

As a different opinion has been lately started, on high authority,* I shall give the proofs of the truth of this my early view, which appear to me of the strictest demonstrative nature.

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* That of M. BECQUEREL.

A solution of nitre, which is a substance neutral to the contact of noble metals, was introduced into a glass cup containing a plate of platinum connected with the multiplier; pure concentrated nitric acid was placed in another cup, in which there was another plate of platinum joined to the other wire of the multiplier, and the connexion was made by a piece of asbestus wetted in a solution of nitre. At the moment of contact, the needle indicated a strong electrical action, negative on the plate plunged in the acid, and which occasioned a permanent deviation of about 60°.

This arrangement was removed from the multiplier, and another substituted for it, in which strong solution of potassa occupied the place of the nitric acid, being in contact with platinum in one cup, and solution of nitre in the other, with the same communications. The deviation was now much weaker, about 10 degrees, and the platinum in the solution of potassa was positive.

The nitric acid and the solution of potassa were now connected in the arrangement by a piece of clean asbestus, moistened in a concentrated solution of nitre; the deviation of the needle was to about 65°. In this instance there was no chemical action of the fluids on each other; for they had no tendency to mix rapidly with the solution of nitre, which being of less specific gravity than either of the other solutions, remained in the asbestus; and there was no effect beyond that of the metallic contact of the platinum with acid and alkali.

A piece of asbestus, of nearly the same size with the other, but dry, was now substituted for the moist asbestus, so that the acid and alkali combined by capillary attraction producing heat; at first, the deviation was rather less than in the former instance; but as soon as the combination was complete, the needle stood exactly at the same point, proving that no electricity was developed by the combination, any more than by the indirect communication of the acid and the alkali.

After trying the effects of the contact of fluid acid upon platinum by the arrangement with solution of nitre, and finding that oxalic acid was the acid among the powerful ones which produced the slightest deviation of the needle, or the smallest negative effect, I employed this acid and solution of potassa, exactly in the same manner as the nitric acid in the experiment just detailed; as the joint action of the acid and alkali on the platinum was only to produce a deviation of 7 or 8 degrees, it might be suspected that any electrical action produced by combination might be more easily manifested; but no such effect occurred; and whether the communication was made by combination through dry asbestus, or through asbestus wetted in a saline solution, the effect was precisely the same.

Again,—the two surfaces of platinum were placed in contact with strong solutions of nitre, and the communication made between them by solution of potassa and nitric acid; there was no electrical action though the chemical combination was intense. But when the fluids were mixed, so that a little acid touched one plate of platinum and a little alkali the other, electro-motion immediately began; and in using muriatic acid and solution of ammonia, which, being lighter than the saline solutions, very soon came in contact with the platinum, the effect commenced almost immediately, and continued for some time to increase.

Again,—I placed pieces of paper coloured with litmus and turmeric, and moistened in solutions of nitre, upon two surfaces of platinum connected with the multiplier; they were covered with a stratum of porcelain clay wetted with the same solution, a stratum of clay moistened with muriatic acid was placed above on one plate, and a stratum moistened with solution of ammonia above on the other, so as to make a contact in which there should be action upon a large surface without direct communication with the metals. In several experiments of this kind there was no electro-motion; and whenever it was perceived it was found that either the acid, or the alkali, or both, had penetrated through the clay, and touched the metals so as to change considerably the colour of the papers, which were placed as indications of the correctness of the experiment.

Having brought forward what appear to me decided proofs on this subject, I shall now proceed to investigate the operation of the metals and fluids in combinations containing two of the latter substances. At first I was surprised to find that platinum acted so powerfully with nitric acid, which undergoes no chemical change by contact with it, and suspecting that it might arise from the presence of minute portions of muriatic acid or muriatic salts, I took great pains to exclude these substances by washing the platinum in distilled water, not touching it with the hands, &c. but when the conditions were those of perfectly clean and pure platinum and perfectly pure nitric acid, the phenomena were the same. Similar reasonings may be applied to solutions of potassa, soda, &c. which do not chemically alter platinum by contact, and yet render it positively electrical with respect to platinum in water or saline solutions. It must however be called to mind that the oxygen in nitric acid, and the metals in the alkalies, have attractions of a very decided kind for platinum; and in taking the scale of electro-negative bodies, solutions of chlorine, or nitro-muriatic acid, produce a more powerful electrical effect on platinum than nitric acid, nitric acid than muriatic, and muriatic than sulphuric.

When platinum is brought in contact with an acid, the pole touching the acid is negative, the opposite pole is positive, as I have found by the condensing electrometer; and the reverse is the case when it touches an alkali, so that the circulation of the electricity is from the metal to the alkali, and from the acid to the metal.

Rhodium, iridium, and gold, act in combinations consisting of acid and alkali, on which they have no chemical effect, exactly like platinum; the surface of the metal in the solution of alkali being positive, that in the solution of the acid, negative. With silver and palladium the electricity is greater, particularly if nitric acid is used; and with charcoal and oxidable metals, there is the same general result, the action being in general exalted in proportion as the chemical attractions are stronger, provided there are no interfering circumstances: and in combinations of this kind nitro-muriatic acid is more active than nitric, and the order is after, nitric, nitrous sulphuric, phosphoric, vegetable acids, sulphurous, prussic, sulphuretted hydrogen, and, with the alkalies, potassa, soda, baryta, ammonia, and so on.

It is always to be understood that strong or concentrated solutions of acids and alkalies are employed; for in cases where the quantity of acid or alkaline matter is very small and the chemical action of the metals strong, there is sometimes a different order. Thus zinc and tin tarnish immediately even in a weak solution of potassa, and, so tarnished, they are negative to the same metals in weak solutions of muriatic or sulphuric acid; but in experiments of this kind it is easy to determine the true circumstances by changing the poles; the negative side, when the energies of the alkali and acid are weak will be determined by the tarnish or coat of oxide formed.

Solutions of sulphurets act in these combinations like alkali, with circumstances depending upon the formation of new compounds according to the law explained in the last section. In combinations, of which the elements are hydro-sulphuret and acid, the metal in the hydro-sulphuretted solution is positive, and that in the acid negative; but with alkalies and hydro-sulphurets, and zinc and tin, the metal in the solution of alkali is positive, and that in the solution of hydro-sulphuret negative: with silver and palladium the opposite order occurs, and with copper there is nearly a balance of powers, or changes of power, dependent upon the circumstances detailed in the last section.

When, in electrical combinations containing one metal, water, or a neutro-saline solution is in one of the cups and alkali or acid in another, the result is usually such as might be anticipated,—the side of the metal in the alkali is positive, that in the acid negative, and that in the neutro-saline solution in the opposite state. There are however certain neutro-saline solutions, which when they contain oxygen or common air, act upon the more oxidable metals, and such have a power or energy of their own; thus zinc, and tin, and copper in solution of common salt, are positive to the same metals in distilled water; and the surfaces of the same metals in weak muriatic acid are positive with respect to the surfaces in water or saline solutions. In combinations, in which weak and strong solutions of acids or of alkalies are the two fluids, both being of the same kind, the electrical action is usually feeble; but the surface in the strongest alkali is most positive, and in the acids the result usually depends upon the nature of the solution; if oxide is formed and deposited, the strongest acid is negative with respect to the diluted one.

The chemical changes produced in combinations of this kind, are best observed in cases where the metals undergo no change; for instance, with platinum, diluted sulphuric acid, and solution of potassa. In this combination, hydrogen soon appears on the platinum in the acid, and a very small quantity of gas, which is probably oxygen, on the platinum in contact with the alkali; and that the acid tends to circulate towards the negative surface, and the alkali towards the positive, is shown by the circumstance of the rapid neutralization of the two menstrua, though separated by asbestus moistened in distilled water.

VI. *Of combinations consisting of two conductors of the more perfect class, and one fluid.*

The order in which metallic bodies exhibit electricities on contact, as is well known, is intimately connected with their relative oxidability, the most oxidable metal being positive with respect to all those below it. This law extends likewise to the newly discovered bases of the alkalies and earths. Potassium and sodium, as I have found by bringing them in contact with zinc in a concentrated solution of alkali, are apparently as much positive with respect to this body, as zinc is with respect to platinum and gold.

There is not however any inherent and specific property in each metal which gives it the electrical character; it depends upon its peculiar state—on that form of aggregation which fits it for chemical change. Thus, zinc in amalgamation with mercury is positive with respect to pure zinc, and the amalgam of tin is in the same state with regard to tin; and the metals of the fixed alkalies in amalgam give the highest positive energy to a mass of mercury some thousands of times their weight.

In general, the electricities developed by metallic contact are of a stronger kind than those resulting from the contact of metals with fluids, so that they are not capable of being changed by them. For instance: zinc in acid is positive with respect to all other metals below it in degree of oxidability, though they are placed in alkalies or solutions of sulphurets: there are however exceptions; for instance, with regard to tin, which, when in a strong solution of potassa is positive to zinc in an acid solution; and with respect to iron, which, though positive with regard to copper in all acid or neutro-saline fluids, is negative to it in solution of sulphurets or of alkalies. The electro-motion in these instances produced by the contact of the fluids prevailing over that produced by the contact of the metals.

And knowing the energies of the acid and alkaline fluids, it is easy to apply them so as to diminish or enhance the electrical effects developed by metallic contact.

If, for instance, in a combination containing zinc and platinum, we use two fluids, and place the acid in contact with the zinc, and the alkali with the platinum, the effect will be exceedingly feeble compared with that produced if the order be reversed, and the zinc be in contact with the alkali, and the platinum with the acid.

The chemical changes taking place in combinations of this kind are always such as tend to restore the equilibrium, the hydrogen and the alkaline body always passing to the negative, and oxygen and the acid to the positive metal.

There is no instance of continued electro-motion except in cases where chemical changes can take place, for even DE LUC's or ZAMBONI's columns do not act when quite dry, and the silver in combinations of this kind, when the negative metal is gold, is uniformly found tarnished: for the exhibition of electricities of tension, however, a very slight chemical action is sufficient, as the quantity of electricity required to give repulsion to light bodies is exceedingly small; but to form electro-magnetic combinations the chemical agents must be of an energetic kind.

As most of the fluids which act powerfully in voltaic combinations containing water, or oxygen and hydrogen, it has been suspected that these principles were essential to the effect; this however does not seem to be the case, for I found zinc and platinum formed powerful electro-motive circles in fused litharge and fused oxy-chlorate of potassa, which are not known to contain water; and I have little doubt that similar effects would be produced by other fused salts containing only acid and alkaline matter.

It may elucidate this part of the subject, which must at best be obscure, to take a view of the changes occurring in one of the simplest voltaic combinations, that consisting of zinc, platinum, and solution of sulphate of soda. It is a fact that zinc and platinum become electrical by contact, the zinc positive, the platinum negative; and the two kinds of electricity are apparently most intense at the surfaces where they are in contact with the fluid, which is too imperfect a conductor to allow them to neutralize or destroy each other: they consequently exert their attractive and repellent powers upon the elements of the menstruum; acid and oxygen circulate to the surface of zinc, which in consequence is dissolved, and alkali and hydrogen to the surface of platinum, of which the hydrogen is disengaged, and the equilibrium broken by the contact of the metals is restored by the chemical changes; so that a constant circulation, or a current of electricity, takes place, the power of the combination becoming feebler in proportion as the solution is decomposed, and acid accumulated round its positive, and alkali round its negative surface.

In cases where acids or acid solutions alone are used, the destruction of one or both surfaces, with the tranfer of hydrogen or oxygen, seems to produce the same effect; and the inactivity of single circles or Voltaic piles, in which pure water is used or saline solutions freed from air, seems to show that the destruction of the surface of the oxidable metal is one of the conditions of continued electrical action; and the cessation of the power of De Luc's or Zamboni's piles, is always connected with the tarnish of the imperfect metal employed in them.

Having published many years ago tables of the electro-chemical relations of metals, which have been copied into many elementary books, I think it proper to give them here in a corrected form with some additions, and the differences dependent upon the nature of the menstruum. The metal mentioned first is positive to all those below it in the scale.

With common acids.

Potassium and its amalgams; barium and its amalgams; amalgam of zinc; zinc; amalgam of ammonium (?); cadmium, tin, iron, bismuth, antimony (?), lead, copper, silver, palladium, tellurium, gold, charcoal, platinum, iridium, rhodium.

With alkaline solutions.

The alkaline metals and their amalgams: zinc, tin, lead, copper, iron, silver, palladium, gold, platinum, &c.

With solutions of hydro-sulphurets.

Zinc, tin, copper, iron, bismuth, silver, platinum, palladium, gold, charcoal.

VII. *On the accumulation of electricity, and the chemical changes it occasions in Voltaic arrangements.*

In the view of electro-motion adopted by the illustrious inventor of the pile, the metals were considered as the *only* agents which, in proportion to their surface and their number, occasioned the constant circulation of a certain quantity of electricity through the fluids, or the connecting wires in the pile; and the chemical changes occurring in these fluids were considered as mere results, and not necessarily connected with the circulation. The inactivity of combinations where no chemical changes occur, is sufficiently hostile to this view; but an examination of some of the circumstances of the construction of compound electrical combinations, will bring this hypothesis, and that which I have ventured to adopt, more distinctly into comparison.

Let a piece of zinc and a piece of platinum, both in glasses filled with a solution of nitrate of potassa, be connected through the multiplier, and let the glasses be joined by asbestus moistened with the same fluid; the needle will mark electrical action: let the two glasses now be joined by an arc composed of zinc and platinum, in such a manner that the order is Voltaic, i. e. that the zinc is opposite to the platinum, in the original combination—the effect will be increased. Now let an arc of pure zinc be introduced; the effect will be less than with the double arc, but superior to that with the asbestus, and the pole of the zinc opposite the platinum will oxidate, and that opposite the zinc will give off hydrogen. Let arcs of other metals be substituted for the zinc; for instance, of tin, of iron, of copper, of silver, of tellurium: the electrical effects will diminish with the oxidability of the metal; and with tellurium, which does not oxidate at the positive pole of a Voltaic battery, they will be destroyed; and the case is the same with rhodium, palladium and platinum. That the effect does not depend upon any circumstance connected with conducting power is evident; for charcoal, which is a very imperfect conductor, acts like an oxidable metal; and a very fine wire of platinum, terminated by a small piece of oxidable metal, acts more efficiently when the oxidable metal is opposite the negative pole, than if the whole chain had been composed of oxidable metal; but entirely destroys the effect when the oxidable metal is opposite the positive pole.

If the contact of the metals only was necessary for continued electro-motion, these results, in which a simple homogeneous chain is interposed between the fluids, would be impossible; but they are a necessary consequence of the electro-chemical theory, in which the destruction of the positive surface by the chemical negative agent is regarded as a necessary condition; and platinum and tellurium acted like zinc, when their surfaces opposite to the platinum were plunged into diluted nitro-muriatic acid.

If two, three, or four glasses are used, and two, three, or four arcs of platinum and zinc, the extreme metals of which are connected through the multiplier, a piece of platinum used instead of one of the arcs will not now entirely destroy the electro-motive effect: it will be diminished as if one arc had been removed. The two will act as a single combination; the three as two arcs, and the four as three; and of course in a Voltaic combination of 100 arcs, a single piece of platinum substituted for any one of the arcs, will diminish the power of the apparatus only $\frac{1}{100}$ part.

In attempting to protect copper by zinc, in a separate vessel, from the action of sea water, I found that when the two vessels were connected by moist tow or vegetable substances, or by a wire (even though fine) of any oxidable metal, the protection was complete: but when even a thick wire of platinum was employed, the copper, though in immediate contact with the zinc, became corroded. After the experiment had continued several days, the surface of the platinum opposite to the copper was found tarnished, as if it had been slightly acted upon by the chlorine combined in the sea water; but this effect had been too feeble to be connected with any sensible degree of electrical polarity in the platinum.

This result, with those mentioned in the preceding pages, seems to show that there can be no accumulation of electricity in Voltaic combinations, unless the same or similar conditions of chemical change exist in the elements or single circles composing them; and that under other conditions, the power generated in single circles is either destroyed or diminished according to the opposing nature, or want of conducting power of the chain of intervening bodies. For instance, in the arrangement (mentioned p. 409) of one piece of zinc and one of platinum, the power is doubled by another series of the same kind, destroyed by an arc of platinum, and diminished by an arc of zinc; by a second solution and a second arc of zinc, it is diminished still more; by a third it is nearly, and by a fourth absolutely, *destroyed.*

As the chemical changes always tend to restore the electrical equilibrium destroyed by the contact of the metals with each other in the fluids, it is evident that in cases in which arcs primarily inactive are connected with those primarily active, the chemical changes produced by the electrical attractions must tend to produce in the primarily inactive parts of the combination an arrangement which must give it a power in direct opposition to that of the primarily active circles; so that when separated, their actions, if any, must be directly the reverse of the other. This result, which I anticipated, I have actually found to be correct; six arcs of platinum in vessels filled with solution of nitre, were connected with a Voltaic battery of 50 pairs of plates; of course each arc gave off oxygen, and collected acid round the pole in the place of the zinc, and afforded hydrogen and collected alkali round the pole in the place of the noble metal: on separating the six arcs from the battery, they were found to possess independent action, the poles which were negative being positive, and those positive being negative: in short, the combination acted as if an original one, consisting of acid, alkali, and platinum.

With arcs of zinc, the results were of the same kind, but the electrical effects were much more distinct: as the tarnished zinc in this case added its own negative power to that produced by the contact with the acid.

In trying similar experiments with six arcs of tin, silver, copper, and other metals, and using different saline solutions, it was found that the reversed electrical effects were most powerful with the most oxidable metals, and the most concentrated and most decomposable solutions; and the weakest arrangement of this kind was with arcs of platinum and pure water; yet even in this instance the water had become slightly alkaline at one pole, and acid at the other.

These experiments, showing the nature of the chemical changes in combinations made active by their connexion with Voltaic batteries, and the influence of the newly developed chemical agents, fully explain the phenomena of the secondary piles of M. RITTER; and combined with the fact, that the metals are not PERFECT conductors for electricities of very low intensity, they offer a simple and adequate solution of the circumstances observed by M. DE LA RIVE on the interposition of different metallic plates in the fluids connecting together Voltaic combinations.*

From the nature of the chemical changes taking place in each single circle of a common Voltaic battery, it is evident, that if any small part of a battery for some time in action, is separated from the whole, and made to act as a distinct combination, its powers must be feebler than if it had been originally an independent series; for the electrical action occasioned by the chemical agents developed in it, are such as to counteract the effects produced by the contact of the metals. Whereas, if a small Voltaic series is connected with a much larger one, in reverse order, its oxidable in the place of the noble metals, though the whole power of the combination is much weakened by it when in union; yet, when separated, it must act with much greater power, as the chemical changes produced are exactly of the kind which must enhance the primary power of the metals. This deduction (a necessary consequence of the electro-chemical theory) I have proved by direct experiment. A series of 6 arcs, composed of zinc and copper and solution of nitre, was connected in the proper order with a Voltaic arrangement of 50 pairs, and suffered to remain in connexion for 10 minutes; they were then separated, and made to act as a single battery: their powers were extremely feeble, not certainly one-third as great as those of a combination of the same kind which had been in action (but unconnected) for the same time. Six arcs of copper and zinc were now connected with the same battery of 50, in a reverse or unconformable manner, so that the six plates of zinc gave off hydrogen and attracted alkali, and the plates of copper oxidated and attracted acid. Being separated after a few minutes, and made to act alone, they exhibited powers which appeared three or four times greater than if they had never been in connexion; the zinc resumed a much higher positive, and the copper a higher negative state, than if they had not before been in the antagonist or unconformable conditions.

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* Annales de Chimie et de Physique, Tom. xxviii. p. 190.

All these facts bear upon the same point, and confirm the view which I took of the nature of Voltaic combinations in the Bakerian Lecture for 1806; in all of which, whether the destruction of the electrical equilibrium is produced by the contact of metals or fluids, it is always restored by chemical changes, and in which the circulation, if it may be so called, depends upon a union of these causes, the direction of the currents being always opposite in the metallic and fluid parts of the combination, so as to produce what may be regarded as an electrical circle.

VIII. *General observations and practical applications.*

To explain the manner in which different chemical agents in combination, and in a perfectly neutral state, instantly start into an active existence, when exposed to the two electrical poles, it is necessary to assume principles, and take views of corpuscular action of a perfectly novel kind; and as the chief agents are invisible, and probably imponderable, no direct demonstrative evidence can be brought forward on the subject; and different hypotheses may in consequence be applied to it. In assuming the idea of two ethereal, subtile, elastic fluids, attractive of the particles of each other, and repulsive as to their own particles, capable of combining in different proportions with bodies, and according to their proportions giving them their specific qualities and rendering them equivalent masses, it would be natural to refer the action of the poles to the repulsions of the substances combined with excess of one fluid, and the attractions of these united to the excess of the other fluid; and a history of the phenomena, not unsatisfactory to the reason, might in this way be made out; but as it is possible likewise to take an entirely different view of the subject, on the idea of the dependence of the results upon the primary attractive powers of the parts of the combination on a single subtile fluid, I shall not enter into any discussion upon this obscure part of theory, but I shall endeavour to clear the way for elucidations of it by stating some experimental results.

Some solution of nitrate of potassa was introduced into a glass basin of six inches in diameter, and large slips of paper, tinged with litmus and turmeric, were placed below the fluid, and connected with two pieces of foil of platinum; so that the indications of the formation of acid and alkali, in any part of the basin, by electricity, would be instant and distinct. The two pieces of foil were now connected with the poles of a Voltaic battery: it was found that the alkali was developed only at the point or immediate surface of the negative platinum, and the acid in the same manner at the surface of the positive platinum; and they then gradually diffused themselves through the fluid in a circle round the conductors, and there was no appearance of any repulsions or attractions of the menstrua in the line of the circuit.

In various repetitions of this experiment the same result was obtained; the alkaline and acid matters were influenced in their direction only by currents produced by the disengaged oxygen or hydrogen, or the inclination of the vessel; in short, by mechanical causes only: and the same effects were produced on the test papers, as if a spherical piece of acid and an amalgam of potassium had been introduced in the places of the two poles.

Mr. Herschel has shown, by some elaborate and ingenious experiments in the last Bakerian Lecture, that an amalgam of potassium, containing so minute a portion as some hundred thousand parts of its weight is strongly attracted so as to occasion violent mechanical motion, by the negative pole in a Voltaic arrangement: and if it be supposed that the fluid is divided into two zones, directly opposite in their powers to the poles of the battery, the virtual change may be regarded as taking place in the two extremities of these zones nearest the neutral point; so that by a series of decompositions and recompositions, the alkaline matters and hydrogen separate at one side, and oxygen, pure or in union, at the other.

In this way, the two electricities may be regarded as the transporters of the ponderable matters, which assume their own peculiar characters at the moment they arrive at the point of rest. I shall detail an experiment which I made under a different form some years ago, and which may assist the imagination in the conception of this singular and mysterious mode of action. A flat glass basin, 10 inches in diameter, was filled with water containing $\frac{1}{2000}$ndth part of its weight of sulphate of potassa, in the bottom of which 30 or 40 separate globules of mercury, containing from 10 to 100 grains each, were placed without any regard to order; two wires of platinum from a battery of 1000 double plates, weakly charged, were made to connect the extremities of the water (passing to the bottom of the basin.) As soon as the electrical communication was made, the globules of mercury in or near the current became instantly agitated; their negative poles became elongated, and approached either the positive pole of the battery, or the positive pole of the contiguous globules of mercury, and streams of oxide flowed with great rapidity from the positive toward the negative pole. No hydrogen appeared at the negative poles of the globules of mercury; but after the action had continued a few minutes, and was then suspended, there was an appearance of some minute globules, owing, as was proved by tests, to the formation and oxidation of potassium which had combined with the mercury, and which, as is evident from Mr. Herschel's researches, had given to that part of the globule in which it had combined its high electro-positive qualities. When the connexion was again made, the same series of constant and violent motions took place; the elongated and negative extremities of every globule moving towards the positive surfaces, and undergoing continual oscillations; but on pouring a small quantity of muriatic acid into the water, so as to make it slightly acid, these phenomena ceased; the masses of mercury resumed their spherical form, hydrogen was given off from the negative surfaces, and all motion and agitation were at an end. The energy of the acid in this case being negative, may be considered as neutralizing the power of the potassium by its immediate contact, and as destroying all the phenomena of attraction by the positive pole.

In the numerous experiments that I made in 1806, on the transfer of acids to the positive pole and of alkalies to the negative pole, there were similar instances in which masses of acid or alkaline matters, by exerting their own peculiar energies, prevented the accumulation of the antagonist elements at their points of rest, so as to destroy, or materially weaken, their power of motion or transport. For instance, in attempting to transfer baryta from the positive to the negative pole, the negative pole being plunged in sulphuric acid, or sulphuric acid to the positive pole, the negative being plunged in a solution of baryta, the re-agents were neutralized, and formed insoluble precipitates at the point of union of the menstrua; and no baryta reached the negative, and no sulphuric acid the positive pole.

With muriatic acid and salts of silver the case was the same. And when acids and alkalies, forming soluble compounds, were used in similar experiments, a great length of time was required, proportional in some measure to their masses, before a particle of acid reached the positive, or of alkali the negative pole; and the result was not destroyed till after the intermediate combination had taken place to a considerable extent; proving the phenomena of continued decompositions and re-compositions, and showing that the electrical and chemical phenomena are of the same order, and produced by the same cause.

In the Bakerian Lecture for 1806, I proposed the electrical powers, or the forces required to dis-unite the elements of bodies, as a test or measure of the intensity of chemical union. By the use of the multiplier it would be now easy to apply this test; and *accurate* researches on the connexion of what may be called the electro-dynamic relations of bodies to their combining masses or proportional numbers, will be the first step towards fixing chemistry on the permament foundation of the mathematical sciences.

I could enter into some other general views of the pure scientific relations of this subject, and its connexion with thermo-electricity and the phenomena of cohesion; but having already taken up so much of the time of the Society, I shall defer what I have to say on these subjects to another occasion, and I shall conclude with a few practical observations.

A great variety of experiments made in different parts of the world has proved the full efficacy of the electro-chemical means of preserving metals, particularly the copper sheathing of ships; but a hope I had once indulged, that the peculiar electrical state would prevent the adhesion of weeds or insects has not been realized; protected ships have often indeed returned after long voyages perfectly bright,* and cleaner than unprotected ships, yet this is not always the case; and though the *whole* of the copper may be preserved ſrom chemical solution in steam vessels by these means, yet they must be adopted in common ships only, so as to preserve a portion,—so applied as to suffer a certain solution of the copper; † and an absolute remedy for adhesions, is to be sought for by other more refined means of protection, and which appear to be indicated by these researches.

The nails used in ships are an alloy of copper and tin, which I find is slightly negative with respect to copper, and it is on these nails that the first adhesions uniformly take place: a slightly positive and slightly decomposable alloy would probably prevent this effect, and I have made some experiments favourable to the idea.

In general, all changes in metals which would indicate the power of chemical attraction, are easily determined by electrical means. Thus I found copper hardened by hammering negative to rolled copper; copper (to use the technical language of manufacturers) both *overpoled* and *underpoled*, containing in one case probably a little charcoal and in the other a little oxide, negative to pure copper. A specimen of brittle copper, put into my hands by Mr. VIVIAN, but in which no impurity could be detected, was negative with respect to soft copper.

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* The Carnebrea Castle.

† A common cause of adhesions of weeds or shell fish, is the oxide of iron formed and deposited round the protectors. In the only experiment in which zinc has been employed for this purpose in actual service, the ship returned after two voyages to the West Indies, and one to Quebec, perfectly clean.

The experiment was made by Mr. Lawrence, of Lombard-street, who in his letter to me states that the rudder, which was not protected, had corroded in the usual manner.

In general, very minute quantities of the oxidable metals render the alloy positive, unless it becomes harder, in which case it is generally negative. As I have mentioned before, amalgams of the oxidable metals are usually positive, not only to mercury, but even to the pure metals.

There are probably few chemical operations which electrical changes do not influence, and either increase or modify. In the rusting of iron, for instance, the oxide formed by the contact of moisture becomes the negative surface, and exalts the oxidability of the mass of metallic iron, and the rust consequently extends in a circle.

The precipitations of metals have been already traced to causes of this kind, and many metallic solutions must belong to the same order of phenomena.

I have pointed out in former papers some of the cases of electro-chemical protection, which I have no doubt, when the principles are well understood, will be generally adopted; and others are constantly occurring. I shall mention one,—the preservation of the iron boilers of steam engines by introducing a piece of zinc or tin. This in the case of steam boats, particularly when salt water is used, may be of the greatest advantage, and prevent the danger of explosion, which generally arises from the wear of one part of the boiler.

Another application of importance which may be made, is the prevention of the wear of the paddles or wheels, which are rapidly dissolved by salt water.

But I will conclude. Whenever a principle or discovery involves or unfolds a law of nature, its applications are almost inexhaustible; and however abstracted it may appear, it is sooner or later employed for common purposes of the arts and the common uses of life.

XXVIII. *On the discordances between the Sun's observed and computed Right Ascensions, as determined at the Blackman-street Observatory, in the years* 1821 *and* 1822; *with Experiments to show that they did not originate in instrumental derangement. Also a description of the seven-feet Transit with which the observations were procured, and upon which the experiments were made. By* James South, *Esq. F. R. S. Communicated June* 1, 1826.

Read June 8, 1826.

In presenting to the Royal Society the following pages, I am well aware that some apology is necessary; the subject however to which they refer being intimately connected with the progress of astronomy, I am induced to hope that the Society will still receive with indulgence, what would long since have been communicated to them, had other astronomical pursuits allowed me the opportunity.

That the sun's right ascension, found by *observation*, frequently disagrees with that afforded by *calculation*, astronomers I believe now generally admit; an opinion however has been as generally entertained, that the discordances were the results of instrumental inaccuracy, occasioned by the effects of the solar rays upon certain parts of the instrument; hence observations of the sun have fallen into disrepute, whenever an accurate knowledge of the time is the object of research.

As, however, there is nothing which more impedes the advancement of science, than opinions too hastily adopted, it may be worth while to inquire whether practical astronomy really merits the above reproach; the investigation will be tedious, but I trust it will be satisfactory.

The transit instrument employed for the purpose was made for me by Mr. TROUGHTON; its object-glass is four inches in clear aperture, its focal length seven feet two inches; and as far as the just proportions of its parts are concerned, it is regarded by him, as his happiest production. Experience having also shown that it is one which future artists will do well to *imitate*, a brief description of it will perhaps be grateful to the Society.

The instrument in its general construction is similar to that of the ten feet transit, which was in the year 1816 erected at the Royal Observatory at Greenwich; there are however some trifling differences, which will be mentioned hereafter.

In Plates XVI. and XVII. figures 1 and 6, the instrument is shown on a scale of one-twelfth of the real dimensions. The telescope (as well as the axis), is formed of conical tubes, the extreme ends of which are determined by the diameter of the object-glass, whilst the larger ends take their dimensions from that of the spherical centre piece, which forms a base for them to rest on. In the two figures just referred to, the centre piece has nearly four-sixths of its surface covered by the four truncated cones of the axis and telescope; but it is not rendered weak by the perforations made in it, those in the direction of the telescope being but a little more than the radius of the object-glass, whilst those in the direction of the axis are no larger than is required to transmit the light of a lamp placed near the end of the axis, uninterruptedly to the central illuminator. The figures 1 and 6 of Plates XVI. and XVII. do not at all show how the four principal parts of the instrument are united to the sphere, but figures 8 and 9 of Plate XVIII. will illustrate a description of what is hitherto *peculiar* to the Greenwich transit instrument and mine.

The ends of all the four cones, where they join the sphere, are strengthened by circular pieces of cast brass; these pieces extend full three inches into the lengths of their respective cones, into which they are soldered and pinned; they are turned concave in front, so as to fit the surface of the sphere, into which they are rabbeted, and serve to keep the opposite branches of the axis and telescope straight, and at right angles with each other. To these brass pieces are attached broad and strong rings, for the reception of the screws which bind the whole together.

The four branches of the axis and telescope are solely united, by what Mr. Troughton calls, *tension bars;* these bars pass through the sphere, six of them in the direction of the axis, and four in that of the telescope. They are arranged at equal distances between corresponding parts, care being taken that those of the axis do not obstruct the rays of the object-glass, and that the illuminator is not shadowed by those of the telescope. The tension bars screw into the rings of the brass pieces above described; they have at one end a fine screw, and at the other a coarse one; the fine one is made about twice as long as under other circumstances would be required: and there are holes in the sphere at proper distances, through which the bars can pass freely.

To connect these various parts, let the fine screw ends of the six bars of the axis be screwed into their proper rings as far as they will go; then pass the bars through the holes in the sphere, and pressing the cone home upon the rabbet retain it there; now address the other cone to the coarse screw ends of the bars, and by turning these in the direction of unscrewing, they will screw into their rings, and bring up the other cone to its bearing, with a power equal to the difference of the ranges of the two screws. The tubes of the telescope are united to the sphere and to each other in the same manner; but to perform this operation, it is necessary to pass the hand into the sphere, for which purpose there are two apertures, with moveable caps left in the middle of its two uncovered parts: the tension bars are acted on by a capstan pin, small holes having been drilled in the bars to receive it. The above caps are covered with platina; on one of them is engraved an inscription, and on the other the maker's name. By the above mode of joining the principal parts, the bars may be stretched, and the sphere* even compressed to any extent short of that, which would occasion a permanent alteration in the length of the former, or in the figure of the latter; a thing which Mr. TROUGHTON considers would perhaps not take place with a force equal to a ton of weight. How much such a connection must be better than any that could be effected by binding together the *exterior* parts, to use the emphatic language of our illustrious artist, "every one who is gifted with mechanical intellect will readily determine."

Plate XVIII. fig. 8, is a section through the axis, and exhibits the six bars which bind together, the cones of the axis, and also the places of the four, which are perpendicular to them, and which connect the tubes of the telescope. In Plate XVIII. fig. 9, which is a section through the telescope, the bars of the telescope are shown lengthwise, whilst those of the axis are perpendicular; in both figures the illuminator is shown, in one the polished surface, the back of the plate in the other; in each it is seen under an angle of 45°, the elliptical perforation appearing as a circle. The removal of the inscription pieces having afforded the draftsman but a limited view of the interior of the sphere, the parts are not represented with precision; but nevertheless may serve well enough to elucidate the preceding description.

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* That every part of the sphere, should possess a power of resistance, as uniform as possible, extreme precaution was employed, in turning its interior surface, so as to render it concentric with the exterior.

In Plate XVII. fig. 6, extending from the cones of the axis to those of the telescope, will be seen four tubes or braces, attached to the former about two inches from the pivots, and to the latter about ten inches from the centre piece; these are so placed as to exert but a very slight pressure, and although deemed by Mr. TROUGHTON *essential* in the Greenwich instrument, were considered *unnecessary* in mine, and for the diminution of expence, would have been omitted but for my interference; in the Greenwich transit they were applied to counteract any disposition to flexure, when the instrument was directed to the horizon; and although the greater length of the Greenwich instrument, would render such an effect more likely to happen than in mine, still, as I had never heard the Astronomer Royal speak but in terms of the highest commendation of *his* instrument, I deemed it consistent with good sense to *profit* by his experience.

Until the Greenwich transit was constructed, the method of placing the telescope to the required altitude, was by means of a semicircle fixed to one of the side pieces, and an index clamped to the pivot of the axis, the vernier of which pressed slightly upon the former. The index in this arrangement is very liable to be disturbed on reversion of the axis, and when the object-end of the telescope accidentally points below the horizon: also, after the index is set, should the position of the telescope be deranged before the observation is commenced, reference must be again made to the divisions of the semicircle; and should the accident occur whilst the star is passing the wires, the observation will be lost. The apparatus to remedy these inconveniences is seen in Plate XVI. and XVII. figs. 1 and 6, but better in Plate XVI. fig. 2, which is drawn to a scale half the dimensions of the original. It consists of two complete circles, firmly attached to the eye-end of the telescope; each circle is provided with two opposite verniers, subdividing its divisions into minutes of a degree; the indices have clamps and slow moving screws, and microscopes are attached to the verniers: a spirit level is also affixed to the index of each circle, whose range of bubble corresponding to one minute, is about half an inch. When this apparatus is adjusted, on the vernier being set to the place of a star, and the telescope moved round till the bubble stand in the middle of its range, then will the star traverse the field between the two horizontal wires.* Hence it is evident, that should by accident the telescope be moved before, or during observation, the merely restoring the bubble to the middle of its range, will again present the star to the observer's view, without any reference to the divisions. But it is often of importance to observe the transits of stars, one of which, in right ascension differs very little with the other; as for instance, Capella and Rigel; here the index of one circle may be set to the first star, whilst that of the other may be placed to the second;

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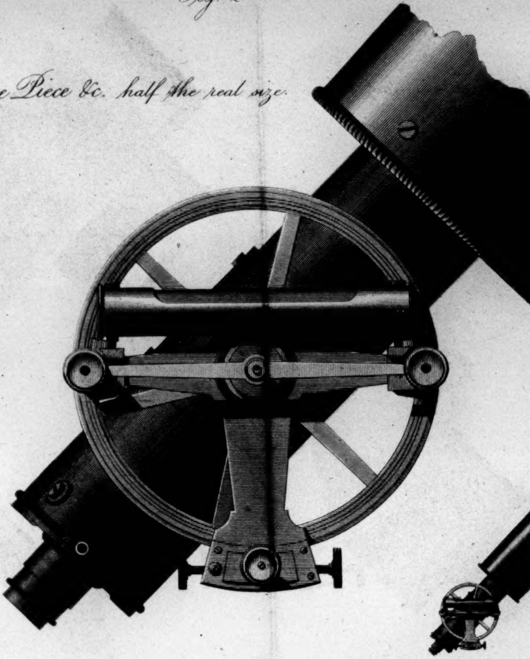
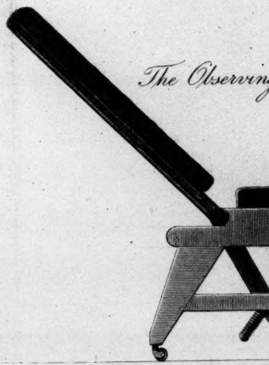
* These wires are distant from each other, about four minutes of a degree.

Fig. 2

Eye Piece &c. half the real size.





The Observing

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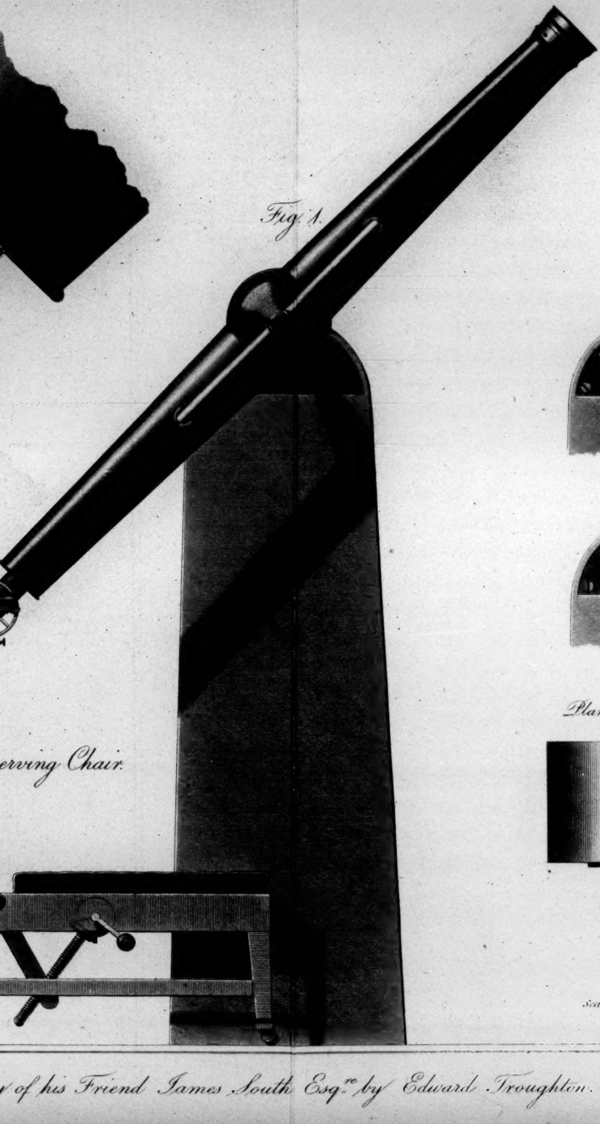
Made for the Observatory of

Fig. 1.



erving Chair.

Fig. 3.

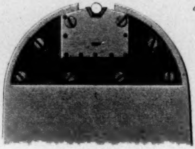
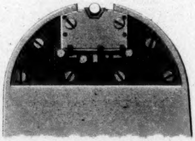
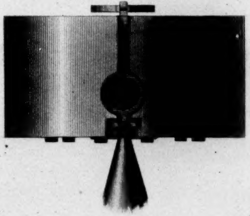


Fig. 4.



Plan of one of the Piers.

Fig. 5.



Scale one Inch to a Foot.

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y of his Friend James South Esq.re by Edward Troughton.

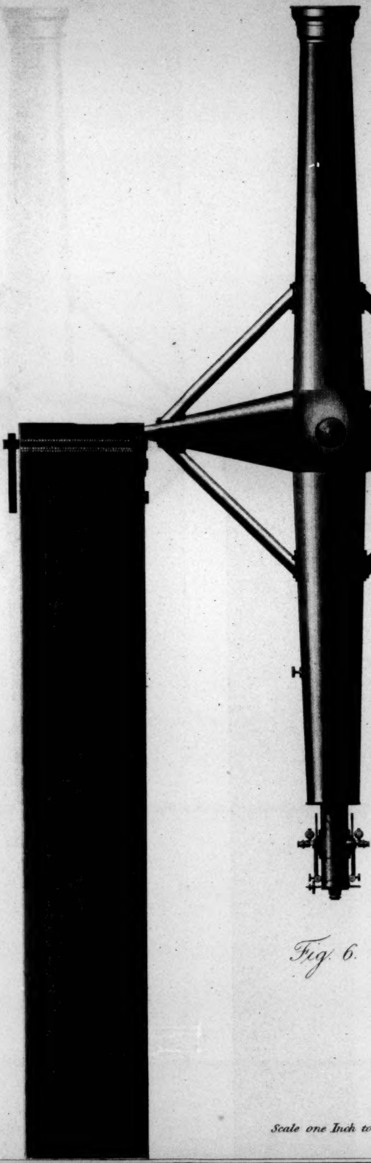

Fig. 6.

Scale one Inch to

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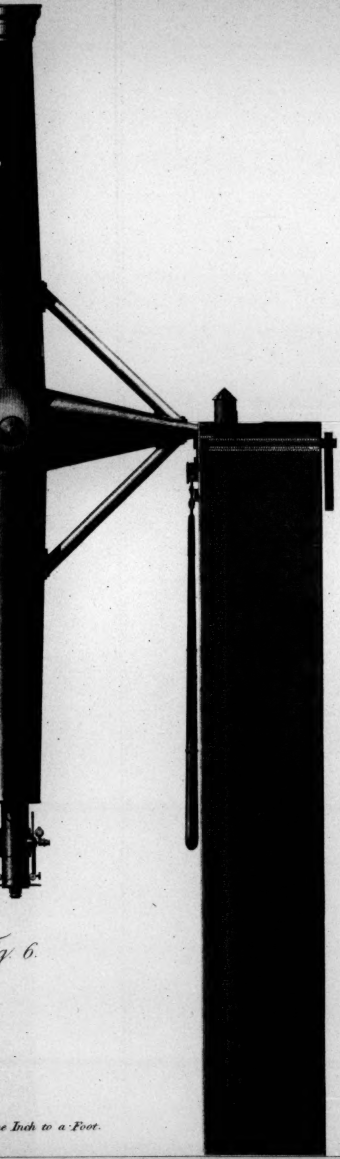
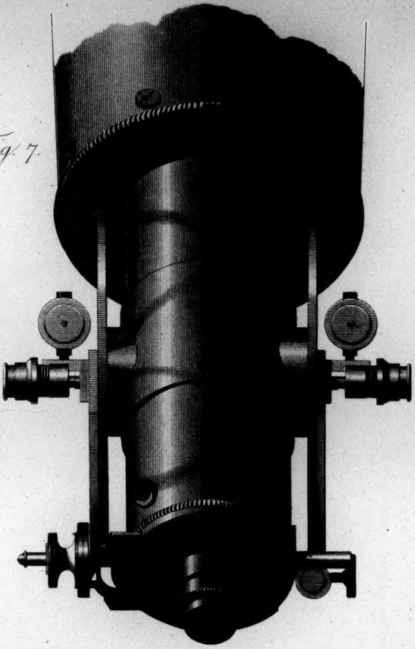


y. 6.

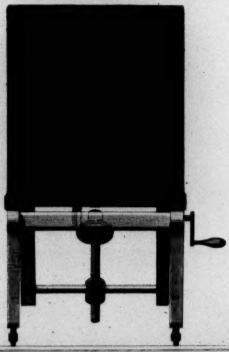
e Inch to a Foot.

Fig. 7.



Front of the Eye Piece.

Back of the Observing Chair.



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Section through the Transverse Axis.

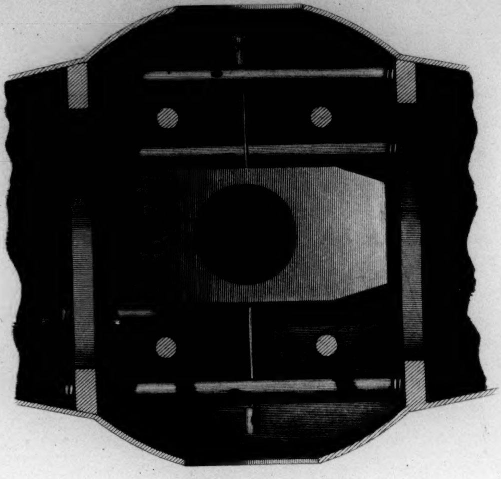

Fig. 8.

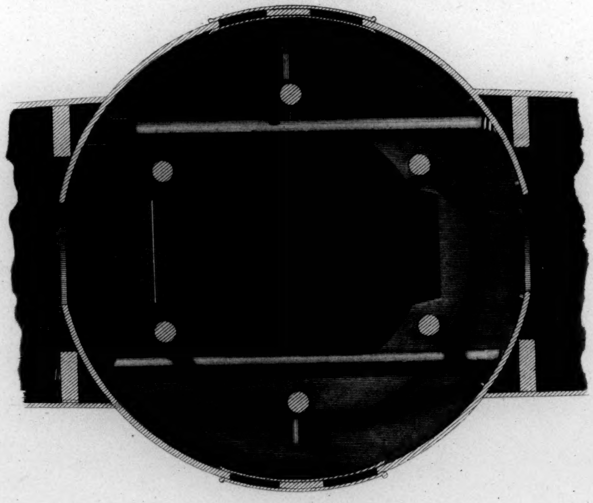

Fig. 9.

Section through the Axis of the Telescope.

Scale of 0 1 2 3 4 5 6 7 8 9 10 11 12 Inches

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J. Basire sc.

and when observations by direct vision, are to be compared with those obtained by reflection, the index of the one, will point the telescope to the direct place of the star, whilst that of the other, will present the instrument to its reflected image.

Figures 3 and 4, of Plate XVI. exhibit the side pieces and Y' in which the pivots of the axis rest; the plates which are semicircular, are imbedded in the stone piers, and are firmly screwed into them. Figure 3, represents the eastern plate, in which the adjustment for the level of the axis is made: a piece, of which the upper end is formed into a Y, is moveable perpendicularly, but well secured from motion in every other direction; the means of gradual adjustment are brought about, by a piece having a short cylindrical part in the middle, at the upper end a fine screw, and at the lower end a coarse one; the fine screw works in the moveable piece, and the coarse one in the fixed plate; the cylindrical part being perforated in many places, enables it to be acted upon by a capstan pin, and thus an effect equal to the difference of the two screws, is produced. This last part, because easy of description, was not brought under the view of the draftsman, by removing the covering plate; a slit in it however exposes two or three of the capstan holes of the differential screw.

Fig. 4, Plate XVI. shows the western plate, the general outline of which corresponds with that just described; the motion of the Y piece is here only horizontal, for the purpose of placing the instrument in the meridian. The adjustment is effected by means of two screws, which work in the opposite sides of the moveable Y piece, and whose heads abut against the fixed plate To produce motion in the Y piece, one of them must be screwed, and the other unscrewed; but in order that the screws might be both moved at the same time, by equal quantities, and when the observer's eye is at the telescope, there is a system of pinion work, the handle for which adjustment is seen in Plate XVII. fig. 6, hanging down close to the inside of the western pier. In the formation of the side plates very great attention was paid to render them steady in themselves, as also that their respective adjustments should not disturb each other.*

Fig. 5. Plate XVI. is a bird's-eye view of the head of one of the piers, and was meant chiefly to show the apparatus for relieving the pivots of the axis, and Y*, from a great part of the weight which would otherwise bear upon them. Immediately behind the adjustable Y piece, but rather broader, is a plain piece of brass, having a Y cut in its upper end; a lever also is seen, one extremity of which passes into a hole made in the Y piece just alluded to, whilst the other end carries a weight; the bar of the lever is expanded into a circle, whose centre is about one-third of the lever's length distant from the pivot of the axis. The circle is sufficiently large to admit the illuminating lantern; in its diameter at right angles to the direction of the lever are inserted two steel screws, whose blunted points are hardened and polished; these rest on hardened and polished steel planes, which are let into the stone pier, and together form the fulcrum in a manner not

unlike the common balance. The weight is a circular thick plate, or short cylinder, and is hooked on to the end of the lever; it is made hollow, with an opening upon it's superior edge, allowing small shot to be introduced at pleasure, according as it is wished that the instrumental portion of the pivot, as also the instrumental Y piece, should be more or less relieved. A reference to fig. 5, Plate XVI. will render all this perfectly intelligible.

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* How completely this desideratum has been attained, it is only necessary to remark, that on the 9th of May, 1822, the western side plate was removed from its pier, in order that Mr. TROUGHTON might apply to it, the pinion work just alluded to; on the 19th of the same month it was returned to me; and although not even an approximate meridian mark was at my command, still, by one observation of the pole star on the same evening, the instrument was placed so nearly in the plane of the meridian, that by the subsequent transits of Arcturus and α Libræ, its ex-meridian position could not be detected.

Fig. 7, Plate XVII. is a perspective view of the eye end of the telescope, in which many of the parts above described are differently, and some of them better seen. In it a micrometer is shown, which moves a plate contiguous to that in which the five transit wires are inserted; one wire is contained in the moveable plate, and is intended to facilitate the observations of Polaris, and other juxta polar stars.

In fig. 6, Plate XVII. on the eastern side of the telescope, is seen projecting from it a finger screw; this gives motion to an apparatus within the tube of the telescope, for regulating the quantity of light projected from the illuminator upon the transit wires of the instrument.

The instrument was placed upon its piers on the 6th of June, 1820, and on the day following a series of experiments was begun, to find, if possible, any defects which might invalidate the accuracy of observations hereafter to be made with it; the permanency of the side plates, and of the Y pieces contained in them, was incessantly scrutinized; observations by reflection and by direct vision were compared; continual reversions of the instrument were made; constant examination of the horizontality of the axis, after every alteration of instrumental position, was never omitted; and the state of its collimation was frequently ascertained.* The results having satisfied my astronomical friends as well as myself, that the instrument fulfilled all the required conditions, further experiments were deemed unnecessary; and on the 5th of August, the instrument being relieved from its two months' "torture," was prepared to grapple with the delicate observations for which it was designed.

The character which the instrument acquired shortly after its erection, four years' subsequent experience has unequivocally confirmed; and exclusive of the property which it is the object of the subsequent pages to investigate, I know not whether most to respect it, for the *unusual* accuracy with which it obeys its adjustments, or for the *extreme* pertinacity with which it retains them.

The object glass of the Greenwich Transit instrument is five inches in clear aperture; its focal length is 10 feet; its horizontal axis, including the pivots, is 3 feet 10 inches; in the focus of the object glass are seven fixed wires, and two moveable for micrometrical purposes; the semicircles at the eye end of the telescope, being insufficient to enable the observer to direct the instrument to the reflected image of a star, a divided circle two feet in diameter, is attached to one end of the axis; the pivots, originally of hard bell metal, having suffered an alteration of figure from constant use, were removed during the spring of last year by Mr. TROUGHTON, and others, made of hardened steel, inserted in their stead. There is no apparatus whereby the observer, whilst making sidereal observations, can communicate to the instrument, azimuthal motion.

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* The proximity of lofty buildings to the north and south of my Observatory, rendering it impossible to erect any object to perform the offices of a meridian mark, an apparatus was planted upon the top of my house, enabling me to examine the collimation, by the flag-staff on Severndroog Castle. The trouble, however, of frequently repeating the operation became so considerable, and from unfavourable state of atmosphere, occasionally so unsatisfactory, that sidereal observations were recurred to, generally of Polaris, and of a small star about 54 minutes from the pole: these, particularly the latter, offer severe tests for the accuracy of the adjustment; and where the instrument can be reversed, without risk of deranging its *horizontality*, (as is the case with *mine*,) no error of collimation, sensible to observation, *need* remain uncorrected.

With these exceptions, the Greenwich Transit is the same as mine; the description therefore given of the one, will convey nearly an accurate idea of the nature of the other.

The computed Right Ascension of the Sun, with which his Right Ascension as determined by observation, will be compared in the subsequent pages, is that given in the Nautical Almanac and Astronomical Ephemeris for the respective years, where it stands computed for the meridian of Greenwich; the comparisons, however, being those arising from observations made at another station, *viz.* Blackman-street Observatory, it becomes necessary to inquire, how far equations can be found, adequate to reduce the sun's right ascension computed for Greenwich, to his right ascension when on the meridian of Blackman-street. This is a matter which observation must decide.

Tables I. and II. show various right ascensions of the sun observed in Blackman-street during the years 1821 and 1822; the former presents sixteen, the latter nineteen transits of the sun made on consecutive days; the maximum difference between the *observed* daily motion in right ascension and the *computed* daily motion in right ascension, is 26 hundredths of a second in the one, and 22 hundredths in the other; in the former table the mean difference of sixteen comparisons is only 4 hundredths of a second in time, whilst in the other it is only 3 hundredths. Hence, there can be no doubt, that we may safely enough employ the computed daily motion in right ascension, to arrive at accurate corrections of the sun's computed right ascension, for the differences of longitude of the two observatories.

Tables III. and IV. contain the sun's right ascension computed for the meridian of Blackman-street, on such days as the sun's transit was observed there, during 1821 and 1822, also the equations employed for the purpose; the longitude of Blackman-street Observatory being 21.76 seconds of time west of the Royal Observatory at Greenwich.

Tables V. and VI. exhibit the difference between the sun's observed and computed right ascensions, as determined in Blackman-street during the years 1821 and 1822; these require explanation.

The Observatory being situated in one of the principal manufacturing, as well as in one of the most populous, districts of the metropolis, the instruments were exposed to the inconveniences of soot falling upon them, from the chimneys of the neighbouring houses, steam engines, &c.; and the transit, from the nature of the opening in the roof, came in for its full share: to protect its tubes, therefore, from the ravages of the soot, they were, shortly after the erection of the instrument, covered with green woollen cloth, which being neatly fitted and attached by buttons, afforded no incumbrance during the observations. The openings in the roof to the north and south were about 18 or 20 inches in breadth; and the telescope, when directed to the zenith, extended some way between the ceiling of the observatory and its roof. The shutters were so contrived as to be opened in an instant; and by a slight frame-work it was very easy to screen all the parts of the instrument, and also the piers, from the access of the sun's rays; it was likewise a matter of the greatest facility to prevent his rays from falling on the eastern half of the instrument, whilst the western was exposed to their influence.

Previous to observing the sun's transit, it was my ordinary habit not to open the shutter, till his first limb had nearly reached the first wire of the instrument. This precaution was uniformly adopted, in the observations of 1821, till the 22d of August; the consequences are seen by the annexed differences.

If, however, we adopt the hypothesis, that the mere exposure of the instrument to the sun's rays during the observation of his transit (a period about 4½ minutes) be adequate to produce instrumental derangement corresponding to 8 or 9 tenths of a second in time, it is fair to expect that a longer exposure would produce a greater discordance, and vice versâ. On the 22d of August, therefore, the western* half of the instrument was exposed to the solar rays, 18 minutes before the sun's centre came to the meridian; the effect, however, being very inconsistent with theory, on the 23d it was exposed 24 minutes; the mean differences of temperature of the western and eastern axes, and western and eastern braces being 14 degrees, but without any evidence of increased displacement.

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* By the nature of the roof, and the construction of the interior of the observatory, independently of the shutter and screen, the sun's rays could not fall on the eastern brace and axis, 'till the sun had nearly reached the meridian; but the western brace and axis, towards the pivot, were accessible to his rays nearly 1½ hour before noon, provided the shutter was opened.

On the 24th of August, the western half of the instrument was exposed 65 minutes before noon, still without any material difference; indeed, if the observations could be relied upon (which they certainly cannot), to 7-hundredths of a second of time, the result of this day's exposure of the instrument, would militate against the hypothesis, that the sun's rays have any thing to do with the matter, seeing that the difference is in the negative sense.

On September 2nd, all the coverings were removed from the instrument, and it was defended from the solar rays, till the sun's first limb had nearly reached the first wire. On September 3rd, the instrument* without its coverings, was exposed to the sun's rays, 59 minutes before his centre came to the meridian; the difference between the thermometers on the western axis and brace, and those on the eastern, being nearly 14 degrees, yet the discordance between the results of the two days transits, is absolutely insensible.

On September the 4th, the instrument was entirely defended from the sun's rays. On the 5th, the western braces and axis, also the western half of the centre piece being covered with black cloth, whilst those on the eastern half were enveloped in white, the instrument was exposed 65 minutes before noon, to the sun's rays; thermometers placed under the covers of the western axis, and western brace, stood 13°.5 higher, than those placed under the covers of the eastern axis and brace; yet the discordance between the observed and computed right ascensions, varies only one thousandth of a second, from the quantity obtained on the 4th, when the instrument was entirely defended from the solar rays.

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* Previously to the shutter being opened, for the experiments of exposure, the instrument was always elevated to the sun's altitude; and it remained so, until the transit was observed: during the experiments, the windows and door of the observatory were closed; the thermometers employed, were made by Mr. TROUGHTON.

On September 24th, the instrument was completely screened from the sun's rays; but on the 25th they were allowed to fall upon the instrument's western axis and brace, sixty-three minutes, during a cloudless sky;* yet between the results of the one day, and the other, there is only a difference of 7 hundredths of a second.

On October 21st and 22nd, the instrument being exposed to the sun's rays, thermometers under the black covers of the western axis and brace, differed on the former day 12°.5 from those under the covers of the white axis and brace; but on the latter, the difference of temperature was more than 16°; the difference between the results of the two days' observations, is nine hundredths of a second: unfortunately, there are no observations with which these can be compared.

In like manner might we discuss individually, the results of experiments made on several occasions, during the year 1822; the days however are noted in Table VI. when the instrument was exposed; and Table VII. details all the particulars which are essential to the investigation; to which therefore the reader is referred, as also for a more circumstantial account of the exposure of the instrument to the sun's rays, during the year 1821.

On looking down the columns of differences, between the *observed* and *computed* right ascensions of the sun, from the various determinations of 1821 and 1822, exhibited in Tables V. and VI. it will be seen, although the difference is not constant, yet that within two or three days, its amount does not greatly vary; by collecting therefore consecutive transits, in pairs, each of which shall always contain a result, derived from observation made during exposure of the instrument, we may probably arrive at some conclusion, which, although not demonstrative, will still merit considerable confidence. Let us begin with 1821.

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* Experiments during exposure of the instrument, were never commenced, except under every probability of success; when however (as frequently happened), transient clouds obscured the sun, even but for half a minute, the operations were discontinued, and the results disregarded and destroyed.

From Table V. 1821.

Instrument exposed.		Instrument defended.		Difference.
	seconds.		seconds.	seconds.
August 22	+ 0.755	August 21	+ 0.733	+ 0.022
Sept. 3	+ 0.672	Sept. 2	+ 0.661	+ 0.011
—— 3	+ 0.672	—— 4	+ 0.661	+ 0.011
—— 5	+ 0.660	—— 4	+ 0.661	— 0.001
—— 25	+ 0.701	—— 24	+ 0.773	— 0.072
		Mean diff. of the 5 pairs =		— 0.0058

From Table VI. 1822.

Instrument exposed.		Instrument defended.		Difference.
	seconds.		seconds.	seconds.
March 1	+ 0.030	February 28	+ 0.225	— 0.195
May 21	+ 0.861	May 22	+ 0.932	— 0.071
—— 31	+ 0.971	June 1	+ 0.826	+ 0.145
June 2	+ 0.826	June 1	+ 0.826	+ 0.000
—— 2	+ 0.826	June 3	+ 0.873	— 0.047
—— 4	+ 0.704	June 3	+ 0.873	— 0.169
—— 7	+ 0.705	June 6	+ 0.927	— 0.222
Decem. 22	+ 0.164	Decem. 23	+ 0.158	+ 0.006
		Mean diff. of the 8 pairs =		— 0.0691

Hence, in 1821, the mean of 5 observations, obtained when the instrument was exposed to the sun's rays, varies from the mean of 5 observations, made when the instrument was entirely defended from their influence, six thousandths of a second of time; whilst in 1822, the mean derived from 8 observations made under exposure, compared with the mean of 8 results, obtained when the instrument was completely defended from the sun's rays, differs sixty-nine thousandths of a second of time.

The mean therefore of the two series, allowing each, a weight proportional to the number of observations on which it rests, is forty-five thousandths of a second of time. Whether this arise, from error of observation, erroneous computation, or from instrumental derangement, we have not sufficient data* to determine: fortunately, however, the quantity is very small, and if it really *could* be brought, to support the hypothesis, " that the sun's rays falling unequally upon the instrument, occasioned the discordances complained of, " it would lose much of its apparent weight, when it is remembered, that not the ordinary exposure of the instrument to the sun, but ten times that quantity, was employed to procure it.

The mean difference however between the observed and computed right ascensions is *less* under exposure, than when the instrument was defended; hence, were it wanted, it might be called upon as additional evidence, in favour of the conclusion which the experiments afford, namely, " that the discordances between the *observed* and *computed* right ascensions, as determined by the Blackman-street observations of 1821 and 1822, were *not* the consequences of instrumental inaccuracy."

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* On referring to page 446, there seems some reason to believe, that the differences found between the observations of February 28th and March 1st, May 31st and June 1st, June 3rd and June 4th, June 6th and June 7th, are *not* the results of instrumental derangement, *nor* of erroneous observation. The Greenwich and Paris observations corroborate our 1st difference; the mean of the Greenwich and Paris, supports our 2d; the Paris determination coincides with our 3d; and the Dublin is nearly similar to our 4th.

To obtain however these results, we have been obliged to recur to the sun's right ascension, as computed in the Nautical Almanac; it is therefore possible, that the near coincidences above indicated, may arise from a balance of errors, between derangement of the instrument on the one hand, and inaccurate calculation on the other; we will therefore appeal to experiments, which shall be independent of astronomical tables.

The brightness of the pole star, and the difference of polar distance between it, and the sun, render it visible in the day time, throughout the year: during the spring and autumn, it comes to the meridian about noon; in the former, at its superior, in the latter, at its inferior transit; in the one instance, the sun is about 8° north, in the other as much south of the equator; the arc therefore intercepted between the star and the sun, being about 20° greater in autumn, than in the spring, observations of the star, will be gotten with greater facility in the former, than in the latter.

With the ordinary observing power of 250, the transit of the star, when *very* steady, may be determined by my instrument, to half a second of time. If therefore, the sun's rays can occasion such instrumental derangement, as may be easily perceptible by the sun's transit, we must expect that their power will be incontrovertibly established, if observations of the pole star, made under exposure of the instrument to the sun's rays, be compared with those made, when the instrument is defended from them.

Table VIII. shows the observed transits of the pole star, during the autumns of 1821 and 1823; also the nature and extent of the exposure, to which the instrument was subjected.

Table IX. indicates the portions of time, in which the pole star passed to the several wires, when the instrument was exposed to the sun's rays; whilst Table X. gives the like information, when the instrument was entirely defended from them.

Table XI. shows the difference between the intervals of time, in which the pole star passed to the several wires, when the instrument was exposed to, and defended from, the sun's rays; and the results for two adjoining wires, are as follow:

Seconds.	
− 0.17	From the observations of 1821.
− 1.25	
− 1.00	
+ 0.17	
+ 1.25	
− 1.00	
− 0.87	
+ 0.57	From the observations of 1823.
− 0.33	
+ 0.50	
+ 0.50	

Mean = − 0.15

Thus it seems, that the time taken by the pole star to pass over any two adjoining wires of the instrument, is *less* when the instrument is exposed to the sun's rays, than when it is defended from them, by 0.15 of a second; which, when quadrupled, and referred to the sun's mean polar distance, is less than two hundredths of a second of time.

The quantity is nearly insensible; and considering that an exposure at least ten times as great, as the instrument receives during an ordinary observation of the sun's transit, was required to produce it, I am led to the conclusion, "that the discordances between the *observed* and *computed* right ascensions of the Sun, as determined by the Blackman-street observations of 1821 and 1822, did *not* arise, from instrumental derangement."

But it may be urged that, although the experiments here narrated, prove that the differences between the sun's observed and computed right ascensions, cannot have arisen from derangement of the instrument employed in obtaining them, still there may be some peculiarity in the eye, or the judgement of the observer, which, if it exist at all, will exist as well in the observations made during exposure, as in those made, when the instrument was defended. This is a point which must be cleared up. If the differences really be as great, as my observations make them, it is fair to expect they cannot have escaped detection, in other observatories. As being easily accessible, and better known in this country than any other, let us appeal to the corresponding observations, at the Royal Observatory of Greenwich, the Royal Observatory of Paris, and the Dublin Observatory.

Our Astronomer Royal, having very obligingly transmitted me a copy of such corresponding observations, as were procured at Greenwich, the comparison with the Blackman-street determinations, is extremely easy, the same mean right ascensions of the standard stars, as also the same corrections, having been used at the two stations.

The Paris and Dublin results, will require reductions to render them comparable with the observations of Blackman-street, and Greenwich; Mons^{r}. BOUVARD having, however, kindly annexed to the Paris observations, the names of the stars used each day, in determining the clock's error, and having also put me in possession of the catalogue used at the Paris observatory, to find equations, by which each observation might be expressed in terms of the Greenwich catalogue, became only a matter of calculation.

Dr. BRINKLEY having likewise been equally indulgent, the Dublin observations are, by similar treatment, available to my purpose.

Tables XII. and XIII. contain the sun's right ascension computed for the meridian of Paris, on such days as the sun's transit was observed at the Paris, and Blackman-street observatories, during 1821 and 1822; whilst Tables XIV. and XV. answer the same purpose for the meridian of Dublin. The longitude of the former, being assumed as $9^{\text{min.}}$ $21^{\text{sec.}}$ of time east; whilst that of the latter is regarded as $25^{\text{min.}}$ $22^{\text{sec.}}$ of time, west of the Royal Observatory at Greenwich.

Tables XVI. and XVII. exhibit the sun's right ascension, as observed at Paris, by the Paris Catalogue, in values of the Greenwich Catalogue; and Tables XVIII. and XIX. serve the like purpose to the Dublin observations, reduced by the Dublin Catalogue.

Tables XX. and XXI. show the differences between the sun's observed and computed right ascensions, by Greenwich observations of 1821 and 1822.

Tables XXII. and XXIII. indicate the differences by Paris observations; and Tables XXIV. and XXV. exhibit the discordances by Dublin observations.

Table XX. shows us, that the mean of 31 observations made at Greenwich in 1821, gave the observed right ascension of the sun, greater than his computed right ascension, 0.627 of a second of time.

And Table XXI. informs us, that by the mean of 45 observations, made at Greenwich in 1822, the observed right ascension, was found greater than the computed, 0.420 of a second.

Table XXII. presents us with the mean of 16 observations of the sun, made at the Royal Observatory of Paris in 1821, whereby his observed right ascension, exceeds his computed right ascension, 0.584 of a second.

And Table XXIII. indicates, that by 28 observations made at the Paris Observatory in 1822, the observed right ascension, was found greater than the computed right ascension, 0.558 of a second.

Table XXIV. offers to our notice, 9 observations of the sun, made in the year 1821, at Dublin; whereby the observed right ascension, was determined to be greater than the computed, 0.666 of a second.

And Table XXV. exhibits 15 observations made in 1822, at the Observatory of Dublin, giving the observed right ascension of the sun, greater than his computed, by 0.686 of a second of time.

The two following Tables will facilitate the comparison of the results, as obtained at the respective observatories.

Table exhibiting the Discordances between the Sun's observed and computed Right Ascensions, as determined at the Observatories of Blackman-street, Greenwich, Paris, and Dublin.

1821.

		Blackman-street.	Greenwich.	Paris.	Dublin.
		seconds.	seconds.	seconds.	seconds.
June	30	+ 0.834	+ 0.780	....	+ 0.939
July	9	0.926	0.830	....	....
	12	0.805	0.690	....	0.864
	18	1.062	0.770	+ 0.275	....
	19	0.978	0.580	0.532	0.811
August	3	0.756	0.670	....	....
	4	0.654	0.470	....	....
	10	0.897	0.770	....	....
	11	0.771	0.470	....	....
	17	0.594	0.830	....	....
	20	0.990	0.660	0.674	....
	21	0.733	0.940	0.411	....
	22	0.755	0.720	0.688	....
	23	0.753	0.450	0.916	0.662
	24	0.679	0.570	0.822	0.869
September	2	0.661	0.840	0.812	....
	3	0.672	0.690	....	....
	4	0.661	0.650	0.648	....
	5	0.660	0.350	0.667	....
	12	0.803	....	....	....
	15	0.681	0.570	....	....
	16	0.808	....	....	....
	24	0.773	....	....	....
	25	0.701	....	....	....
October	2	0.795	0.550	0.415	0.357
	21	0.729	0.890	0.918	....
	22	0.640	0.670	....	....
	29	0.529	0.410	0.445	0.501
	30	0.608	0.640	0.330	....
November	6	0.566	0.610	0.366	....
December	2	0.576	....	....	....
	4	0.623	0.610	+ 0.417	....
	5	0.423	0.090	....	....
	6	0.564	0.900	....	0.569
	8	0.453	0.370	....	....
	11	+ 0.503	+ 0.380	....	+ 0.419

Table exhibiting the Discordances, between the Sun's observed and computed Right Ascensions, as determined at the Observatories of Blackman-street, Greenwich, Paris, and Dublin.

1822.

	Blackman-street.	Greenwich.	Paris.	Dublin.
	seconds.	seconds.	seconds.	seconds.
January 15	+ 0.476	+ 0.180	+ 0.072	....
16	0.500	0.440	....	....
February 21	0.603	0.340	0.637	....
23	0.379	0.230	0.370	....
24	0.579	0.000	....	....
28	0.225	+ 0.150	0.610	....
March 1	0.030	— 0.100	0.436	....
April 30	0.715	+ 0.300	0.173	....
May 1	0.705	0.230	0.466	+ 0.672
21	0.861	0.790	0.490	0.913
22	0.932	0.590	1.084	0.865
24	0.597	0.550	....	0.777
27	0.983	0.670	....	....
31	0.971	0.330	0.771	....
June 1	0.826	0.480	0.233	0.630
2	0.826	0.570	0.627	....
3	0.873	0.430	0.819	0.806
4	0.704	0.460	0.631	....
6	0.927	0.680	0.606	1.007
7	0.705	0.320	1.137	0.727
22	0.694	0.570	....	0.752
July 4	0.879	....	0.605	....
7	1.095	0.700	....	....
10	0.974	....	1.081	....
31	0.889	0.430	....	....
August 1	0.670	0.310	0.972	....
2	0.812	0.570	0.868	0.722
3	0.736	0.820	....	....
4	0.697	0.600	....	....
8	0.834	0.420	....	....
9	0.864	....	....	....
17	0.665	0.460	0.384	....
18	0.743	0.420	0.351	....
19	0.563	0.150	....	0.535
21	0.576	0.540	....	0.372
October 14	0.633	....	....	....
November 4	0.430	0.320	....	....
9	0.367	0.000	....	....
13	0.342	0.310	....	....
14	0.432	0.320	....	0.301
26	0.305	....	0.412	....
29	0.467	0.480	....	....
December 6	0.543	0.540	0.383	....
7	0.525	0.470	0.667	0.658
8	0.395	0.590	....	....
22	0.164	0.370	0.121	....
23	0.158	0.280	....	....
26	0.372	0.410	0.159	+ 0.552
28	0.279	0.390	+ 0.447	....
30	+ 0.492	+ 0.800	....	....

Hence we find for 1821,

seconds.

That 31 observations made in Blackman-street, gave the sun's observed right ascension, greater than the computed, - - - - - 0.708

And by 31 correspondent observations at the Royal Observatory of Greenwich, the observed right ascension, was found greater than the computed, 0.627

That 16 observations made in Blackman-street, gave the sun's observed right ascension, greater than the computed, - - - - - - 0.736

And that 16 observations on corresponding days, made at the Royal Observatory of Paris, determined the observed right ascension, to exceed the computed, - - - - - - 0.584

That 9 observations made in Blackman-street, found the sun's observed right ascension, greater than his computed right ascension, - - - 0.716

And that 9 correspondent observations made at Dublin, found the observed right ascension, greater than the computed, - - - 0.666

And during 1822,

seconds.

That 45 observations made in Blackman-street, determined the sun's observed right ascension, to be greater than the computed, - - - - 0.608

And that 45 observations made at the Royal Observatory at Greenwich, on corresponding days, gave the observed right ascension, greater than the computed by, - - - - - 0.420

seconds.

That 28 observations made at the Observatory in Blackman-street, found the observed right ascension, to exceed the computed, - - - 0.632

And that by 28 corresponding observations at the Royal Observatory of Paris, the observed right ascension, was determined to be greater than the computed, - - - - - - 0.558

That by 15 observations in Blackman-street, the observed right ascension, was found greater than the computed, by - - - - - 0.693

And that 15 correspondent observations made at the Observatory of Dublin, the observed right ascension, exceeded the computed, by - - 0.686

Seeing therefore that results not materially differing from the Blackman-street determinations, are derived from the Greenwich, the Paris, and the Dublin observations, it is reasonable to conclude, that the discordances between the observed and computed right ascensions of the sun, as found by the Blackman-street observations, did not arise from any peculiarity in the eye, or judgement of the individual employed, in obtaining them.

We have however hinted in a former part of this memoir, that the differences as determined in Blackman-street, were not constant; and by reference to the preceding tables, discordances amongst them, to an amount far greater than can be attributed to erroneous observation, will readily be detected; hence, an investigation into their nature, becomes desirable; this, however, would lead us into an inquiry beyond the purport of the present communication; which, besides a brief description of an admirable instrument, was intended chiefly to show, " that the discordances between the *observed* and *computed* Right Ascensions of the Sun, as determined at the Blackman-street Observatory, in the years 1821 and 1822, did *not* originate, in *instrumental* inaccuracy."

I hope however ere long to show, to the satisfaction of the Society, that the source of the discordances, must be sought for, in the imperfections of the Solar Tables.

JAMES SOUTH.

Sloane-street, N°. 132.
May 24, 1826.

Table I.

To show the Differences which exist, between the Sun's observed daily motion in Right Ascension, and his computed daily motion in Right Ascension; (by Blackman-street observations.)

1821.

		Sun's observed Right Ascension.	Observed daily motion in R. A.	Computed daily motion in R.A.	Diff. of the observed and comp^d.
		h. m. s.	m. s.	m. s.	s.
July	18	7 49 40.623	4 0.916	4 1.000	−0.084
	19	7 53 41.539			
August	3	8 52 49.015	3 51.598	3 51.700	−0.102
	4	8 56 40.613			
	10	9 19 38.255	3 47.274	3 47.400	−0.126
	11	9 23 25.529			
	20	9 57 8.647	3 42.143	3 42.400	−0.257
	21	10 0 50.790			
	—	10 0 50.790	3 41.921	3 41.900	+0.021
	22	10 4 32.711			
	—	10 4 32.711	3 41.398	3 41.400	−0.002
	23	10 8 14.109			
	—	10 8 14.109	3 41.026	3 41.100	−0.074
	24	10 11 55.135			
September	2	10 44 47.316	3 37.411	3 37.400	+0.011
	3	10 48 24.727			
	—	10 48 24.727	3 37.189	3 37.200	−0.011
	4	10 52 1.916			
	—	10 52 1.916	3 36.899	3 36.900	−0.001
	5	10 55 38.815			
	15	11 31 37.636	3 35.527	3 35.400	+0.127
	16	11 35 13.163			
	24	12 3 57.928	3 36.028	3 36.100	−0.072
	25	12 7 33.956			
October	21	13 43 7.287	3 47.511	3 47.600	−0.089
	22	13 46 54.798			
	29	14 13 47.288	3 53.380	3 53.300	+0.080
	30	14 17 40.668			
December	4	16 42 19.690	4 21.200	4 21.400	−0.200
	5	16 46 40.890			
	—	16 46 40.890	4 21.941	4 21.800	+0.141
	6	16 51 2.831			
					Mean = −0.040

Table II.

To show the Differences which exist, between the Sun's observed daily motion in Right Ascension, and his computed daily motion in Right Ascension; (by Blackman-street observations.)

1822.

		Sun's observed Right Ascension.	Observed daily motion in R. A.	Computed daily motion in R. A.	Diff. of the observed and comp^{d}.
		h. m. s.	m. s.	m. s.	s.
January	15	19 47 13.042	4 17.523	4 17.500	+ 0.023
	16	19 51 30.565			
February	23	22 24 59.537	3 48.100	3 47.900	+ 0.200
	24	22 28 47.637			
	28	22 43 52.682	3 44.705	3 44.900	— 0.195
March	1	22 47 37.387			
April	30	2 28 32.973	3 48.390	3 48.400	— 0.010
May	1	2 32 21.363			
	31	4 30 53.633	4 4.955	4 5.100	— 0.145
June	1	4 34 58.588			
	—	4 34 58.588	4 5.401	4 5.400	+ 0.001
	2	4 39 3.989			
	—	4 39 3.989	4 5.947	4 5.900	+ 0.047
	3	4 43 9.936			
	—	4 43 9.936	4 6.031	4 6.200	— 0.169
	4	4 47 15.967			
	6	4 55 29.590	4 7.078	4 7.300	— 0.222
	7	4 59 36.668			
August	1	8 44 8.529	3 53.042	3 52.900	+ 0.142
	2	8 48 1.571			
	—	8 48 1.571	3 52.224	3 52.300	— 0.076
	3	8 51 53.795			
	—	8 51 53.795	3 51.761	3 51.800	— 0.039
	4	8 55 45.556			
	8	9 11 6.692	3 48.830	3 48.800	+ 0.030
	9	9 14 55.522			
	17	9 45 6.422	3 44.078	3 44.000	+ 0.078
	18	9 48 50.500			
	—	9 48 50.500	3 43.320	3 43.500	— 0.180
	19	9 52 33.820			
November	13	15 12 33.205	4 5.690	4 5.600	+ 0.090
	14	15 16 38.895			
December	6	16 49 59.310	4 22.282	4 22.300	— 0.018
	7	16 54 21.592			
	—	16 54 21.592	4 22.770	4 22.900	— 0.130
	8	16 58 44.362			
	22	18 0 41.032	4 26.594	4 26.600	— 0.006
	23	18 5 7.626			
					Mean = — 0.030

Table III.

To reduce the Sun's Right Ascension, computed for the meridian of Greenwich, to the meridian of Blackman-street.

1821.

		Sun's R. A. computed for the meridian of Greenwich.	Computed daily motion in R. A.	Correction for diff. of long.	Sun's R. A. computed for the meridian of Blackman-street Observatory.
		h. m. s.	m. s.	sec.	h. m. s.
June	30	6 36 3.500	4 8.5	+ 0.063	6 36 3.563
July	9	7 13 8.400	4 5.3	0.062	7 13 8.462
	12	7 25 23.000	4 4.0	0.062	7 25 23.062
	18	7 49 39.500	4 1.0	0.061	7 49 39.561
	19	7 53 40.500	4 0.5	0.061	7 53 40.561
August	3	8 52 48.200	3 51.7	0.059	8 52 48.259
	4	8 56 39.900	3 51.1	0.059	8 56 39.959
	10	9 19 37.300	3 47.4	0.058	9 19 37.358
	11	9 23 24.700	3 46.9	0.058	9 23 24.758
	17	9 45 57.900	3 43.7	0.057	9 45 57.957
	20	9 57 7.600	3 42.4	0.057	9 57 7.657
	21	10 0 50.000	3 41.9	0.057	10 0 50.057
	22	10 4 31.900	3 41.4	0.056	10 4 31.956
	23	10 8 13.300	3 41.1	0.056	10 8 13.356
	24	10 11 54.400	3 40.6	0.056	10 11 54.456
September	2	10 44 46.600	3 37.4	0.055	10 44 46.655
	3	10 48 24.000	3 37.2	0.055	10 48 24.055
	4	10 52 1.200	3 36.9	0.055	10 52 1.255
	5	10 55 38.100	3 36.7	0.055	10 55 38.155
	12	11 20 50.600	3 35.5	0.055	11 20 50.655
	15	11 31 36.900	3 35.4	0.055	11 31 36.955
	16	11 35 12.300	3 35.4	0.055	11 35 12.355
	24	12 3 57.100	3 36.1	0.055	12 3 57.155
	25	12 7 33.200	3 36.3	0.055	12 7 33.255
October	2	12 32 51.600	3 38.0	0.056	12 32 51.656
	21	13 43 6.500	3 47.6	0.058	13 43 6.558
	22	13 46 54.100	3 48.1	0.058	13 46 54.158
	29	14 13 46.700	3 53.3	0.059	14 13 46.759
	30	14 17 40.000	3 54.1	0.060	14 17 40.060
November	6	14 45 15.200	3 59 7	0.061	14 45 15.261
December	2	16 33 38.000	4 20.2	0.066	16 33 38 066
	4	16 42 19.000	4 21.4	0.067	16 42 19.067
	5	16 46 40.400	4 21.8	0.067	16 46 40.467
	6	16 51 2.200	4 22.4	0.067	16 51 2.267
	8	16 59 47.500	4 23 3	0.067	16 59 47.567
	11	17 12 58.800	4 24.5	+ 0.067	17 12 58.867

Note. In computing these corrections, it seems, that I used 22 seconds, in lieu of 21.76, as the longitude of my observatory; the consequence is immaterial.

Sloane Street, July 22d, 1826.

Table IV.

To reduce the Sun's Right Ascension, computed for the meridian of Greenwich, to the meridian of Blackman-street.

1822.

		Sun's R. A. computed for the meridian of Greenwich.			Computed daily motion in R. A.		Correction for diff. of long.	Sun's R. A. computed for the meridian of Blackman-street Observatory.		
		h.	m.	s.	m.	s.	s.	h.	m.	s.
January	15	19	47	12.500	4	17.5	+ 0.066	19	47	12.566
	16	19	51	30.000	4	16.9	0.065	19	51	30.065
February	21	22	17	21.500	3	49.1	0.058	22	17	21.558
	23	22	24	59.100	3	47.9	0.058	22	24	59.158
	24	22	28	47.000	3	47.2	0.058	22	28	47.058
	28	22	43	52.400	3	44.9	0.057	22	43	52.457
March	1	22	47	37.300	3	44.3	0.057	22	47	37.357
April	30	2	28	32.200	3	48.4	0.058	2	28	32.258
May	1	2	32	20.600	3	48.8	0.058	2	32	20.658
	21	3	50	26.700	4	0.3	0.061	3	50	26.761
	22	3	54	27.100	4	0.9	0.061	3	54	27.161
	24	4	2	29.500	4	1.9	0.062	4	2	29.562
	27	4	14	36.600	4	3.4	0.062	4	14	36.662
	31	4	30	52.600	4	5.1	0.062	4	30	52.662
June	1	4	34	57.700	4	5.4	0.062	4	34	57.762
	2	4	39	3.100	4	5.9	0.063	4	39	3.163
	3	4	43	9.000	4	6.2	0.063	4	43	9.063
	4	4	47	15.200	4	6.5	0.063	4	47	15.263
	6	4	55	28.600	4	7.3	0.063	4	55	28.663
	7	4	59	35.900	4	7.5	0.063	4	59	35.963
	22	6	1	50.900	4	9 6	0.064	6	1	50.964
July	4	6	51	36.200	4	7.2	0.063	6	51	36.263
	7	7	3	56.800	4	6.3	0.063	7	3	56.863
	10	7	16	14.400	4	5.1	0.062	7	16	14.462
	31	8	40	14.200	3	53.6	0.059	8	40	14.259
August	1	8	44	7.800	3	52.9	0.059	8	44	7.859
	2	8	48	0.700	3	52.3	0.059	8	49	0.759
	3	8	51	53.000	3	51.8	0.059	8	51	53.059
	4	8	55	44.800	3	51.1	0.059	8	55	44.859
	8	9	11	5.800	3	48.8	0.058	9	11	5.858
	9	9	14	54.600	3	48.3	0.058	9	14	54.658
	17	9	45	5.700	3	44.0	0.057	9	45	5.757
	18	9	48	49.700	3	43.5	0.057	9	48	49.757
	19	9	52	33.200	3	43.0	0.057	9	52	33.257
	21	9	59	58.700	3	42.0	0.057	9	59	58.757
October	14	13	15	59.700	3	43.1	0.057	13	15	59.757
November	4	14	36	21.200	3	57.8	0.061	14	36	21.261
	9	14	56	19.000	4	2.1	0.062	14	56	19.062
	13	15	12	32.800	4	5.6	0.063	15	12	32.863
	14	15	16	38.400	4	6.4	0.063	15	16	38.463
	26	16	6	49.100	4	15.9	0.065	16	6	49.165
	29	16	19	38.900	4	18.1	0.066	16	19	38.966
December	6	16	49	58.700	4	22.3	0.067	16	49	58.767
	7	16	54	21.000	4	22.9	0 067	16	54	21.067
	8	16	58	43.900	4	23.4	0.067	16	58	43.967
	22	18	0	40.800	4	26.6	0.068	17	0	40.858
	23	18	5	7.400	4	26.6	0.068	18	5	7.468
	26	18	18	26.900	4	26.3	0.068	18	18	26.968
	28	18	27	19.400	4	25.9	0.068	18	27	19.468
	30	18	36	11.000	4	25.6	+ 0.068	18	36	11.068

Table V.

To show the difference between the Sun's observed and computed Right Ascensions; (by Blackman-street observations).

1821.

		Sun's R. A. observed when on the meridian of Blackman-street Observatory.			Sun's R. A. computed for the meridian of Blackman-street Observatory.			Diff. of the observed and comp. R. A.	
		h.	m.	s.	h.	m.	s.	s	
June	30	6	36	4.397	6	36	3.563	+ 0.834	During these observations, the instrument was defended from the sun's rays, till his first limb had nearly reached, the first wire.
July	9	7	13	9.388	7	13	8.462	0.926	
	12	7	25	23.867	7	25	23.062	0.805	
	18	7	49	40.623	7	49	39.561	1.062	
	19	7	53	41.539	7	53	40.561	0.978	
August	3	8	52	49.015	8	52	48.259	0.756	
	4	8	56	40.613	8	56	39.959	0.654	
	10	9	19	38.255	9	19	37.358	0.897	
	11	9	23	25.529	9	23	24.758	0.771	
	17	9	45	58.551	9	45	57.957	0.594	
	20	9	57	8.647	9	57	7.657	0.990	
	21	10	0	50.790	10	0	50.057	0.733	
	22	10	4	32.711	10	4	31.956	0.755	exposed
	23	10	8	14.109	10	8	13.356	0.753	exposed
	24	10	11	55.135	10	11	54.456	0.679	exposed
September	2	10	44	47.316	10	44	46.655	0.661	defended
	3	10	48	24.727	10	48	24.055	0.672	exposed
	4	10	52	1.916	10	52	1.255	0.661	defended
	5	10	55	38.815	10	55	38.155	0 660	exposed
	12	11	20	51.458	11	20	50.655	0.803	defended
	15	11	31	37.636	11	31	36.955	0.681	defended
	16	11	35	13.163	11	35	12.355	0.808	defended
	24	12	3	57.928	12	3	57.155	0.773	defended
	25	12	7	33.956	12	7	33.255	0.701	exposed
October	2	12	32	52.451	12	32	51.656	0.795	defended
	21	13	43	7.287	13	43	6.558	0.729	exposed
	22	13	46	54.798	13	46	54.158	0.640	exposed
	29	14	13	47.288	14	13	46.759	0.529	defended
	30	14	17	40.668	14	17	40.060	0.608	defended
November	6	14	45	15.827	14	45	15.261	0.566	defended
December	2	16	33	38.642	16	33	38.066	0.576	defended
	4	16	42	19.690	16	42	19.067	0.623	defended
	5	16	46	40.890	16	46	40.467	0.423	defended
	6	16	51	2.831	16	51	2.267	0.564	defended
	8	16	59	48.020	16	59	47.567	0.453	defended
	11	17	12	59.370	17	12	58.867	+ 0.503	defended
							Mean by 36 obs, =	+ 0.712	

Note; wherever the word " defended " is annexed to the column of differences, in this, and the following tables, it means, that every part of the instrument, except the object-glass, was entirely excluded from the sun's rays, during the day of observation; as were also the side plates and stone piers.

Table VI.

To show the Differences between the Sun's observed and computed Right Ascensions; (by Blackman-street observations).

1822.

		Sun's R. A. when obs^{d} on the meridian of the Blackman-st. Observ^{y}.			Sun's R. A. computed for the meridian of Blackman-street.			Diff. of the observed and computed R. A.	
		h.	m.	s.	h.	m.	s.	s.	
January	15	19	47	13.042	19	47	12.566	+ 0.476	defended
	16	19	51	30.565	19	51	30.065	0.500	defended
February	21	22	17	22.161	22	17	21.558	0.603	defended
	23	22	24	59.537	22	24	59.158	0.379	defended
	24	22	28	47.637	22	28	47.058	0.579	defended
	28	22	43	52.682	22	43	52.457	0.225	defended
March	1	22	47	37.387	22	47	37.357	0.030	exposed
April	30	2	28	32.973	2	28	32.258	0.715	defended
May	1	2	32	21.363	2	32	20.658	0.705	defended
	21	3	50	27.622	3	50	26.761	0.861	exposed
	22	3	54	28.093	3	54	27.161	0.932	defended
	24	4	2	30.159	4	2	29.562	0.597	defended
	27	4	14	37.645	4	14	36.662	0.983	defended
	31	4	30	53.633	4	30	52.662	0.971	exposed
June	1	4	34	58.588	4	34	57.762	0.826	defended
	2	4	39	3.989	4	39	3.163	0.826	exposed
	3	4	43	9.936	4	43	9.063	0.873	defended
	4	4	47	15.967	4	47	15.263	0.704	exposed
	6	4	55	29.590	4	55	28.663	0.927	defended
	7	4	59	36.668	4	59	35.963	0.705	exposed
	22	6	1	51.658	6	1	50.964	0.694	exposed
July	4	6	51	37.142	6	51	36.263	0.879	defended
	7	7	3	57.958	7	3	56.863	1.095	defended
	10	7	16	15.436	7	16	14.462	0.974	defended
	31	8	40	15.148	8	40	14.259	0.889	defended
August	1	8	44	8.529	8	44	7.859	0.670	defended
	2	8	48	1.571	8	48	0.759	0.812	defended
	3	8	51	53.795	8	51	53.059	0.736	defended
	4	8	55	45.556	8	55	44.859	0.697	defended
	8	9	11	6.692	9	11	5.858	0.834	defended
	9	9	14	55.522	9	14	54.658	0.864	defended
	17	9	45	6.422	9	45	5.757	0.665	defended
	18	9	48	50.500	9	48	49.757	0.743	defended
	19	9	52	33.820	9	52	33.257	0.563	defended
	21	9	59	59.333	9	59	58.757	0.576	defended
October	14	13	16	0.390	13	16	59.757	0.633	defended
November	4	14	36	21.691	14	36	21.261	0.430	defended
	9	14	56	19.429	14	56	19.062	0.367	defended
	13	15	12	33.205	15	12	32.863	0.342	defended
	14	15	16	38.895	15	16	38.463	0.432	defended
	26	16	6	49.470	16	6	49.165	0.305	defended
	29	16	19	39.433	16	19	38.966	0.467	defended
December	6	16	49	59.310	16	49	58.767	0.543	defended
	7	16	54	21.592	16	54	21.067	0.525	defended
	8	16	58	44.362	16	58	43.967	0.395	defended
	22	18	0	41.032	18	0	40.868	0.164	exposed
	23	18	5	7.626	18	5	7.468	0.158	defended
	26	18	18	27.340	18	18	26.968	0.372	defended
	28	18	27	19.747	18	27	19.468	0.279	defended
	30	18	36	11.560	18	36	11.068	+ 0.492	defended

Mean by 50 obs. = + 0.620

Table VII.

To show the nature and extent of the exposure, to which the Instrument was subjected.

1821.

Exposure of the Instrument, August 22.

The Sun's rays were allowed to fall upon the Instrument, eighteen minutes before his centre came to the meridian; not a cloud intervened, during the interval of exposure; no thermometers were appealed to.

Exposure of the Instrument, August 23.

Times of Comparison. h. m.	Thermometers on the Western Axis.	Thermometers on the Eastern Axis.
9 46	69°.0	69°.0
9 54	83 .9	70 .3
9 57	85 .0	71 .0
10 0	85 .6	71 .0
10 5	85 .6	71 .2
10 10	88 .0	72 .0
	Mean = 85 .6	Mean = 71 .1

Hence, difference of temperature = 14°.5.

Exposure of the Instrument, August 24.

Times of Comparison. h. m.	Thermometers on the Western Axis.	Thermometers on the Eastern Axis.
9 8	70°.0	70°.1
9 18	79 .2	71 .8
9 24	83 .0	71 .8
9 29	84 .5	71 .9
9 33	85 .0	71 .9
9 38	85 .9	71 .9
	Mean = 83 .5	Mean = 71 .9

Hence, difference of temperature = 11°.6.

Table VII.—*continued.*

The bulbs of the thermometers, were now placed under the covers* of each axis and brace, and the results were as follow.

Times of Comparison h. m	Thermometers on the Western Axis.	Thermometers on the Eastern Axis.
9 57	88°.0	72°.0
10 1	90 .0	72 .1
10 6	93 .0	72 .3
10 13	95 .8	72 .5
	Mean = 91 .7	Mean = 72 .2

Hence, difference of temperature = 19°.5.

Thermometers under the covers of the braces, afforded results nearly the same as the above. Not a cloud passed over the sun, during the experiments.

Exposure of the Instrument, September 3.

Times of Comparison. h. m.	Thermometer on the Western Axis.	Thermometer on the Eastern Axis.
10 33	80°.0	69°8.
10 36	83 .2	70 .4
10 39	84 .6	71 .0
10 42	84 .8	72 .1
	Mean = 83 .1	Mean = 70 .8

Hence, difference of temperature = 13°.7.

Note.—The instrument during this day's experiments, was deprived of all its coverings. The exposure commenced at 9h 43′ sidereal time, but no comparison of the thermometers was made, until 10h 33′.

Exposure of the Instrument, September 5.

To procure more decisive differences of temperature, between the western brace and axis, and those on the eastern side of the instrument, the former, were now enveloped in black woollen cloth, the latter, in white; the western half also of the centre piece, was covered with *black*, whilst the eastern half of it was enclosed, in *white* cloth; the telescope tubes, however,

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* Vide page 434.

Table VII.—*continued.*

were still included in their ordinary coverings, of green cloth.* These arrangements were persevered in, during all future observations; the different portions were well fitted to the figure of the instrument, and not being unseemly, were constantly retained in sitû.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
9 45	68°.0	68°.0
10 1	78.0	69.8
10 7	81.8	70.3
10 10	82.2	70.6
10 13	84.5	70.8
10 21	84.0	70.8
10 30	85.5	71.5
10 35	84.0	71.8
10 38	84.0	71.8
10 45	87.0	72.2
10 49	89.0	72.5
10 51	90.0	72.4
	Mean = 84.5	Mean = 71.3

Hence, difference of temperature = 13°.2.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Brace.	Thermometers under the cover of the White, or Eastern Brace.
9 45	68°.0	68°.0
10 23	84.0	71.0
10 25	84.5	72.0
10 30	85.5	72.0
10 35	85.5	72.0
10 40	86.0	71.0
10 45	86.0	73.0
10 49	88.0	73.0
10 51	89.5	73.0
	Mean = 86.1	Mean = 72.1

Hence, difference of temperature = 14°.0.

During these experiments not a cloud had been visible.

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* Vide page 434.

Table VII.—*continued.*

Exposure of the Instrument, September 25.

The Sun's rays were allowed to fall upon the instrument, sixty-three minutes before his centre came to the meridian; other observations prevented me, attending to the thermometers; the sky cloudless.

Exposure of the Instrument, October 21.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
12 56	50°.0	50°.0
13 9	59 .0	51 .5
13 10	60 .5	51 .5
13 12	62 .0	51 .7
13 19	65 .0	52 .0
13 23	67 .0	52 .2
13 27	69 .0	52 .5
13 45	70 .0	53 .5
	Mean = 64 .6	Mean = 52 .1

Hence, difference of temperature = 12°.5.

Thermometers placed under the covers of the black and white braces, did not vary half a degree, from those applied to the cones of the axis. During the observations, not a cloud was visible.

Exposure of the Instrument, October 22.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
12 56	51°.0.	51°.0
13 6	64 .0	52 .5
13 12	65 .0	53 .0
13 18	67 .0	53 .5
13 20	68 .5	53 .8
13 22	71 .0	54 .0
13 24	73 .8	54 .0
13 26	74 .5	54 .1
13 46	76 .0	54 .8
	Mean = 70 .0	Mean = 53 .7

Hence, difference of temperature = 16°.3.

Thermometers under the covers of the black, and white braces, afforded results differing from the above, only a small fraction of a degree.

Table VII.—*continued.*

1822.

Exposure of the Instrument, March 1.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
21 33	41°.0	41°.0
22 10	58 .0	44 .0
22 27	68 .5	44 .5
22 40	68 .0	44 .5
22 49	68 .5	45 .5
	Mean = 65 .8	Mean = 44 .6

Hence, difference of temperature = 21°.2.

Not a cloud was visible, from the time at which the shutter was opened, until the experiments were concluded.

Exposure of the Instrument, May 21.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Brace.	Thermometers under the cover of the White, or Eastern Brace.
2 47	66°.3	66°.3
3 22	89 .0	72 .0
3 45	91 .0	74 .0
3 53	94 .0	74 .0
	Mean = 91 .3	Mean = 73 .3

Hence, difference of temperature = 18°.0.

Thermometers under the covers of the western and eastern axes, gave results similar to these. Sky cloudless.

Exposure of the Instrument, May 31.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
3 20	60°.0	60°.0
3 55	80 .0	62 .0
4 10	84 .0	63 .0
4 20	86 .0	63 .0
4 33	90 .0	64 .0
	Mean = 85 .0	Mean = 63 .0

Hence, difference of temperature = 22°.0.

A cloudless sky, during the observations. Thermometers under the covers of the braces, gave results coincident with the above.

Table VII.—*continued.*

Exposure of the Instrument, June 2.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
3 30	67°.5	67°.8
4 0	80.0	70.0
4 5	86.0	71.0
4 15	95.0	73.0
4 28	96.0	74.0
4 34	98.0	74.5
4 42	100.5	75.0
	Mean = 92.6	Mean = 72.9

Hence, difference of temperature = 19°.7.

Thermometers under the covers of the braces, afforded results similar with the above. Not a cloud passed over the Sun, during the time the instrument was exposed to his rays. Thermometers placed *immediately over* the black axis, never indicated a temperature exceeding 94°.

Exposure of the Instrument, June 4.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
3 50	68°.5	68°.3
4 25	96.0	71.0
4 35	98.0	72.0
4 40	100.0	74.0
4 45	104.0	76.0
4 50	107.0	76.0
	Mean = 101.0	Mean = 73.8

Hence, difference of temperature = 27°.2.

Thermometers placed under the covers of the braces, do not differ one degree from the above. A cloudless sky.

Table VII.—*continued.*

Exposure of the Instrument, June 7.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Brace.	Thermometers under the cover of the White, or Eastern Brace.
3 50	62°.0	62°.0
4 20	90 .0	64 .0
4 30	96 .0	65 .0
4 50	100 .0	68 .0
5 2	108 .0	74 .0
	Mean = 98 .5	Mean = 67 .8

Hence, difference of temperature = 30°.7.

Not a cloud visible, during the exposure of the instrument. Thermometers under the covers of the axis, gave results similar to the above.

Exposure of the Instrument, June 22.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
5 0	61°.3	61°.5
5 20	88 .0	64 .0
5 30	90 .0	64 .0
5 45	92 .0	66 .0
5 53	94 .0	68 .0
6 4	98 .0	71 .0
	Mean = 92 .4	Mean = 66 .6

Hence, difference of temperature = 25°.8.

Exposure of the Instrument, December 22.

The Sun's rays, were allowed to fall upon the instrument, half an hour before noon, at which time, the thermometers on the western and eastern axes, stood at 53°.0 and 31°.0.

Hence, difference of temperature = 22°.0.

Table VIII.

To show the Transits of the Pole Star, and the nature of the exposure to which the Instrument was subjected.

1821.

Observed Transits of the Pole Star.

	m. s.	m. s.	h. m. s.	m. s.	m. s.	
October 20	A 28 40.0	B 43 1.5	C 0 57 20.0	D 11 39.5	E 26 1.5	Polaris.
21	E 28 38.0	D 43 0.0	C 12 57 19.0	B 11 35.0	A 25 55.0	Polaris *sp.* (trem^{s}.)
22	E 28 34.0	D 42 58.5	C 12 57 17.5	B 11 35.5	A 25 55.0	Polaris *sp.*
22	A 28 42.0	B 43 3.0	C 0 57 20.5	D 11 38.0	E 26 2.0	Polaris.
24	E 28 34.5	D 42 55.0	C 12 57 14.0	B 11 29.5	A	Polaris *sp.*

On the 21st and 22nd, the Sun's rays were allowed to fall upon the instrument, immediately after the star, at its *sub* polar transit, had passed the wire C, until its transits over the wires B and A, were procured.

With these exceptions, the instrument was entirely defended, from the influence of the solar rays.

Exposure of the Instrument, October 21.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
12 56	50°.0	50°.0
13 9	59 .0	51 .5
13 10	60 .5	51 .5
13 12	62 .0	51 .7
13 19	65 .0	52 .0
13 23	67 .0	52 .2
13 26.30	69 .0	52 .5
	Mean = 63 .7	Mean = 51 .9

Hence, difference of temperature = 11°.8.

Thermometers placed under the covers of the black and white braces, did not differ with those applied to the axes, half a degree.

During the observations not a cloud visible.

Table VIII.—*continued.*

Exposure of the Instrument, October 22.

Times of Comparison.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
h. m.	°	°
12 56	51 .0	51 .0
13 6	64 .0	52 .5
13 12	65 .0	53 .0
13 18	67 .0	53 .5
13 20	68 .5	53 .8
13 22	71 .0	54 .0
13 24	73 .8	54 .0
13 26	74 .5	54 .1
	Mean = 69 .1	Mean = 53 .6

Hence, difference of temperature = 15°.5.

Thermometers placed under the covers of the black and white braces, gave results similar to these. An Italian sky; not a cloud to be seen.

1823.

Observed Transits of the Pole Star.

	m. s.	m. s.	h. m. s.	m. s.	m. s.	
October 9	......	D 44 11.0	C 12 58 39.0	......	......	Polaris *s p.*
10	......	D 44 14.5	C 12 58 38.0	......	......	Polaris *s p.*
11	......	D 44 15.0	C 12 58 38.0	......	......	Polaris *s p.*
11	A 29 55.5	B 44 21.5	C 0 58 46.0	D 13 11.0	E 27 38.0	Polaris.
12	E 29 43.5	......	....	B 12 59.5	......	Polaris *s p.*
13	A 29 56.0	B 44 21.0	C 0 58 46.0	D 13 13.0	E 27 40.5	Polaris.
15	A 29 53.0	B 44 23.0	C 0 58 47.0	D 13 10.0	E 27 38.0	Polaris.
16	E 29 45.0	D 44 13.0	C 12 58 38.5	......	......	Polaris *s p.*
*16	A 29 56.0	B 44 23.0	C 0 58 44.5	D 13 12.0	......	Polaris.

On the 9th and 10th, the Sun's rays were allowed to fall upon the instrument, after the star at its *sub*-polar transit, had passed the wire D.

On the 12th and 16th, the instrument was exposed to the Sun, after that the star at its *sub*-polar transit, had traversed the wire E.

With these exceptions, the instrument was *entirely defended,* from the Sun's rays.

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* These observations (the clock's daily rate being nearly insensible), indicate slight *ex*-meridian position; and may serve as a practical illustration, of the statement made in page 432; seeing, that *seven* months have elapsed, since the instrument was moved by its azimuthal adjustment; and that nearly *seventeen* have transpired, since non-horizontality of its axis, could be detected.

Table VIII.—*continued.*

Exposure of the Instrument, October 9.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
12 43	54°.0	54°.0
12 49	69 .0	54 .7
12 51	73 .0	54 .8
12 53	75 .0	55 .0
12 55	76 .0	55 .2
12 57	76 .5	55 .8
12 59	78 .0	56 .0
	Mean = 74 .6	Mean = 55 .3

Hence, difference of temperature = 19°.3.

Thermometers placed under the covers of the black and white braces, did not vary from those applied to the axes, more than half, or three-quarters of a degree. During the observations not a cloud had been visible.

Exposure of the Instrument, October 10.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
12 42	50°.0	50°.0
12 49	68 .0	51 .0
12 52	75 .0	52 .0
12 53	78 .0	52 .3
12 55	79 .0	52 .5
12 57	81 .0	53 .0
12 59	83 .0	53 .5
	Mean = 77 .3	Mean = 52 .4

Hence, difference of temperature = 24°.9.

Not a cloud visible, during the observations. Thermometers under the covers of the western and eastern braces, did not differ more than half a degree, from those applied to the axes.

Table VIII.—*continued.*

Exposure of the Instrument, October 12.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
12 28	50°.0	50°.0
12 40	68 .0	51 .5
12 50	70 .0	51 .7
12 53	73 .0	51 .8
12 57	74 .0	52 .2
13 5	74 .5	52 .5
13 10	75 .2	53 .0
13 12	75 .4	53 .2
	Mean = 72 .9	Mean = 52 .3

Hence, difference of temperature = 20°.6.

Thermometers under the covers of the western and eastern braces, did not differ from those applied to the axes, one degree.

Light clouds passing, prevented the transits over the wires D and C, being procured; and it was not deemed right to call in the aid of the micrometer wire, lest any source of error, might be suspected. Not a cloud, however, was visible, south of the zenith of the observatory during the experiments. The transits over E and B, were extremely satisfactory, the star being remarkably steady.

Exposure of the Instrument, October 16.

Times of Comparison. h. m.	Thermometers under the cover of the Black, or Western Axis.	Thermometers under the cover of the White, or Eastern Axis.
12 28	49°.0	49°.0
12 34	64 .5	50 .0
12 38	66 .5	51 .5
12 42	69 .0	51 .7
12 45	70 .0	52 .0
12 49	71 .0	52 .6
12 55	72 .5	53 .0
12 59	73 .8	53 .3
	Mean = 69 .6	Mean = 52 .0

Hence, difference of temperature = 17°.6.

Thermometers under the covers of the braces, gave results not differing from the above, one degree. Not a cloud visible, during the observations.

Table IX.

Showing the time, in which the Pole Star passed to the several wires of the Instrument, under experiments of exposure.

1821.

	min.	sec.		min.	sec.
October 21. From C to B	14	16. 0	October 21. From C to A	28	36. 0
22	14	18. 0	22	28	37. 5
Mean =	14	17. 0	Mean =	28	36.75
October 21. From B to A	14	20. 0	October 21. From B to D	28	35. 0
22	14	20. 5	22	28	37. 0
Mean =	14	20.25	Mean =	28	36. 0
October 21. From B to E	42	57. 0	October 21. From A to D	42	55. 0
22	43	1. 5	22	42	56. 5
Mean =	42	59.25	Mean =	42	55.75

	min.	sec.
October 21. From A to E	57	17.0
22	57	21.0
Mean =	57	19.0

1823.

	min.	sec.		min.	sec.
October 9. From C to D	14	28. 0	October 12. From B to E	43	16.0
10	14	23. 5			
16	14	25. 5			
Mean =	14	25.67			
October 16. From E to D	14	28. 0	October 16. From E to C	28	53.5

Table X.

Showing the times in which the Pole Star passed to the several wires, when the Instrument was defended from the Sun's rays.

1821.

		min.	sec.
October 20.	From C to B	14	18. 5
22		14	17. 5
24		14	15. 5
	Mean =	14	17.17
October 20.	From B to A	14	21. 5
22		14	21. 0
	Mean =	14	21.25
October 20.	From B to E	43	0. 0
22		42	59. 0
24		42	55. 0
	Mean =	42	58. 0
October 20.	From A to E	57	21. 5
22		57	20. 0
	Mean =	57	20.75
October 20.	From C to A	28	40. 0
22		28	38. 5
	Mean =	28	39.25
October 20.	From B to D	28	38. 0
22		28	35. 0
24		28	34. 5
	Mean =	28	35.83
October 20.	From A to D	42	59. 5
22		42	56. 0
	Mean =	42	57.75

1823.

		min.	sec.
October 11.	From C to D	14	23. 0
11		14	25. 0
13		14	27. 0
15		14	23. 0
16		14	27. 5
	Mean =	14	25.10
October 11.	From E to D	14	27. 0
13		14	27. 5
15		14	28. 0
	Mean =	14	27.50
October 11.	From B to E	43	16. 5
13		43	19. 5
15		43	15. 0
	Mean =	43	17.00
October 11.	From E to C	28	52. 0
13		28	54. 5
15		28	51. 0
	Mean =	28	52.50

Table XI.

Showing the differences between the intervals of time, in which the Pole Star passed to the several wires, when the instrument was exposed to, and defended from, the Sun's rays.

1821.		min.	sec.	Difference. sec.
C B	(1 interval); 1 exposed =	14	17.00	— 0.17
C B	defended.......... =	14	17.17	
C A	(2 intervals); 2 exposed =	28	36.75	— 2.50
C A	defended.......... =	28	39.25	
B A	(1 interval); 1 exposed =	14	20.25	— 1.00
B A	defended.......... =	14	21.25	
B D	(2 intervals); 1 exposed =	28	36.00	+ 0.17
B D	defended.......... =	28	35.83	
B E	(3 intervals); 1 exposed =	42	59.25	+ 1.25
B E	defended.......... =	42	58.00	
A D	(3 intervals); 2 exposed =	42	55.75	— 2.00
A D	defended.......... =	42	57.75	
A E	(4 intervals); 2 exposed =	57	19.00	— 1.75
A E	defended.......... =	57	20.75	
1823.		**min.**	**sec.**	**Difference. sec.**
C D	(1 interval); 1 exposed =	14	25.67	+ 0.57
C D	defended.......... =	14	25.10	
B E	(3 intervals); 3 exposed =	43	16.00	— 1.00
B E	defended.......... =	43	17.00	
E D	(1 interval); 1 exposed =	14	28.00	+ 0.50
E D	defended.......... =	14	27.50	
E C	(2 intervals); 2 exposed =	28	53.50	+ 1.00
E C	defended.......... =	28	52.50	

Table XII.

To reduce the Sun's Right Ascension, computed for the meridian of Greenwich, to the meridian of Paris.

1821.

		Sun's R. A. computed for the meridian of Greenwich.			Computed daily motion in R. A.		Correction for Diff. of Longit.	Sun's R. A. computed for the meridian of Paris.		
		h.	m.	s.	m.	s.	s.	h.	m.	s.
July	18	7	49	39.500	4	1.0	—1.565	7	49	37.935
	19	7	53	40.500	4	0.5	1.562	7	53	38.938
August	20	9	57	7.600	3	42.4	1.444	9	57	6.156
	21	10	0	50.000	3	41.9	1.441	10	0	48.559
	22	10	4	31.900	3	41.4	1.438	10	4	30.462
	23	10	8	13.300	3	41.1	1.436	10	8	11.864
	24	10	11	54.400	3	40.6	1.432	10	11	52.968
September	2	10	44	46.600	3	37.4	1.412	10	44	45.188
	4	10	52	1.200	3	36.9	1.408	10	51	59.792
	5	10	55	38.100	3	36.7	1.407	10	55	36.693
October	2	12	32	51.600	3	38.0	1.415	12	32	50.185
	21	13	43	6.500	3	47.6	1.478	13	43	5.022
	29	14	13	46.700	3	53.3	1.515	14	13	45.185
	30	14	17	40.000	3	54.1	1.520	14	17	38.480
November	6	14	45	15.200	3	59.7	1.556	14	45	13.644
December	4	16	42	19.000	4	21.4	—1.697	16	42	17.303

Table XIII.

To reduce the Sun's Right Ascension, computed for the meridian of Greenwich, to the meridian of Paris.

1822.

		Sun's R. A. computed for the meridian of Greenwich.			Computed daily motion in R. A.		Correction for Diff. of Longit.	Sun's R. A. computed for the meridian of Paris.		
		h.	m.	s.	m.	s.	s.	h.	m.	s.
January	15	19	47	12.500	4	17.5	— 1.672	19	47	10.828
February	21	22	17	21.500	3	49.1	1.487	22	17	20.013
	23	22	24	59.100	3	47.9	1.480	22	24	57.620
	28	22	43	52.400	3	44.9	1.460	22	43	50.940
March	1	22	47	37.300	3	44.3	1.456	22	47	35.844
April	30	2	28	32.200	3	48.4	1.483	2	28	30.717
May	1	2	32	20.600	3	48.8	1.486	2	32	29.114
	21	3	50	26.700	4	0.3	1.560	3	50	25.140
	22	3	54	27.100	4	0.9	1.564	3	54	25.536
	31	4	30	52.600	4	5.1	1.591	4	30	51.009
June	1	4	34	57.700	4	5.4	1.593	4	34	56.107
	2	4	39	3.100	4	5.9	1.597	4	39	1.503
	3	4	43	9.000	4	6.2	1.599	4	43	7.401
	4	4	47	15.200	4	6.5	1.601	4	47	13.599
	6	4	55	28.600	4	7.3	1.606	4	55	26.994
	7	4	59	35.900	4	7.5	1.607	4	59	34.293
July	4	6	51	36.200	4	7.2	1.605	6	51	34.595
	10	7	16	14.400	4	5.1	1.591	7	16	12.809
August	1	8	44	7.800	3	52.9	1.512	8	44	6.288
	2	8	48	0.700	3	52.3	1.508	8	47	59.192
	17	9	45	5.700	3	44.0	1.454	9	45	4.246
	18	9	48	49.700	3	43.5	1.451	9	48	48.249
November	26	16	6	49.100	4	15.9	1.662	16	6	47.438
December	6	16	49	58.700	4	22.3	1.703	16	49	56.997
	7	16	54	21.000	4	22.9	1.707	16	54	19.293
	22	18	0	40.800	4	26.6	1.731	18	0	39.069
	26	18	18	26.900	4	26.3	1.729	18	18	25.171
	28	18	27	19.400	4	25.9	— 1.727	18	27	17.673

Table XIV.

To reduce the Sun's Right Ascension, computed for the meridian of Greenwich, to the meridian of Dublin Observatory.

1821.

	Sun's R. A. computed for the meridian of Greenwich.	Computed daily motion in R. A.	Correction for Diff. of Longit.	Sun's R. A. computed for the meridian of Dublin Observatory.
	h. m. s.	m. s.	s.	h. m. s.
June 30	6 36 3.500	4 8.5	+ 4.342	6 36 7.842
July 12	7 25 23.000	4 4.0	4.298	7 25 27.298
19	7 53 40.500	4 0.5	4.237	7 53 44.737
August 23	10 8 13.300	3 41.1	3.895	10 8 17.195
24	10 11 54.400	3 40.6	3.886	10 11 58.286
October 2	12 32 51.600	3 38.0	3.840	12 32 55.440
29	14 13 46.700	3 53.3	4.110	14 13 50.810
December 6	16 51 2.200	4 22.4	4.622	16 51 6.822
11	17 12 58.800	4 24.5	+ 4.659	17 13 3.459

Table XV.

To reduce the Sun's Right Ascension, computed for the meridian of Greenwich, to the meridian of Dublin Observatory.

1822.

	Sun's R. A. computed for the meridian of Greenwich.	Computed daily motion in R. A.	Correction for Diff. of Longit.	Sun's R. A. computed for the meridian of Dublin Observatory.
	h. m. s.	m. s.	s.	h. m. s.
May 1	2 32 20.600	3 48.8	+ 4.030	2 32 24.630
21	3 50 26.700	4 0.3	4.233	3 50 30.933
22	3 54 27.100	4 0.9	4.244	3 54 31.344
24	4 2 29.500	4 1.9	4.261	4 2 33.761
June 1	4 34 57.700	4 5.4	4.323	4 35 2.023
3	4 43 9.000	4 6.2	4.337	4 43 13.337
6	4 55 28.600	4 7.3	4.356	4 55 32.956
7	4 59 35.900	4 7.5	4.360	4 59 40.260
22	6 1 50.900	4 9.6	4.397	6 1 55.297
August 2	8 48 0.700	3 52.3	4.092	8 48 4.792
19	9 52 33.200	3 43.0	3.928	9 52 37.128
21	9 59 58.700	3 42.0	3.911	10 0 2.611
November 14	15 16 38.400	4 6.4	4.340	15 16 42.740
December 7	16 54 21.000	4 22.9	4.631	16 54 25.631
26	18 18 26.900	4 26.3	+ 4.673	18 18 31.573

Table XVI.

To convert the Sun's observed Right Ascension, reduced by the Paris Catalogue, into his correspondent Right Ascension, by the Greenwich Catalogue.

1821.

		Sun's R. A. observed at Paris, the reductions being made by the Paris Catalogue.			Equation for Diff. of Paris and Greenwich Catal.	Sun's R. A. observed at Paris, reduced by the Greenwich Catalogue.		
		h.	m.	s.	s.	h.	m.	s.
July	18	7	49	37.950	+0.260	7	49	38.210
	19	7	53	39.200	0.270	7	53	39.470
August	20	9	57	6.550	0.280	9	57	6.830
	21	10	0	48.620	0.350	10	0	48.970
	22	10	4	30.830	0.320	10	4	31.150
	23	10	8	12.440	0.340	10	8	12.780
	24	10	11	53.530	0.260	10	11	53.790
September	2	10	44	45.750	0.250	10	44	46.000
	4	10	52	0.210	0.230	10	52	0.440
	5	10	55	37.080	0.280	10	55	37.360
October	2	12	32	50.280	0.320	12	32	50.600
	21	13	43	5.600	0.340	13	43	5.940
	29	14	13	45.330	0.340	14	13	45.630
	30	14	17	38.540	0.270	14	17	38.810
November	6	14	45	13.680	0.330	14	45	14.010
December	4	16	42	17.460	+0.260	16	42	17.720

Table XVII.

To convert the Sun's observed Right Ascension, reduced by the Paris Catalogue, into his correspondent Right Ascension, by the Greenwich Catalogue.

1822.

		Sun's R. A. observed at Paris, the reductions being made by Paris Catalogue.			Equation for Diff. of Paris and Greenwich Catal.	Sun's R. A. observed at Paris, reduced by the Greenwich Catalogue.		
		h.	m.	s.	s.	h.	m.	s.
January	15	19	47	10.500	+ 0.400	19	47	10.900
February	21	22	17	20.310	0.340	22	17	20.650
	23	22	24	57.710	0.280	22	24	57.990
	28	22	43	51.240	0.310	22	43	51.550
March	1	22	47	35.980	0.300	22	47	36.280
April	30	2	28	30.710	0.180	2	28	30.890
May	1	2	32	19.280	0.300	2	32	19.580
	21	3	50	25.350	0.280	3	50	25.630
	22	3	54	26.230	0.390	3	54	26.620
	31	4	30	51.430	0.370	4	30	51.780
June	1	4	34	56.150	0.190	4	34	56.340
	2	4	39	1.910	0.220	4	39	2.130
	3	4	43	7.950	0.270	4	43	8.220
	4	4	47	14.030	0.200	4	47	14.230
	6	4	55	27.410	0.190	4	55	27.600
	7	4	59	35.070	0.360	4	59	35.430
July	4	6	51	34.850	0.350	6	51	35.200
	10	7	16	13.500	0.390	7	16	13.890
August	1	8	44	6.980	0.280	8	44	6.260
	2	8	47	59.730	0.340	8	48	0.060
	17	9	45	4.300	0.330	9	45	4.630
	18	9	48	48.260	0.340	9	48	48.600
November	26	16	6	47.570	0.280	16	6	47.850
December	6	16	49	57.020	0.360	16	49	57.380
	7	16	54	19.610	0.350	16	54	19.960
	22	18	0	38.910	0.280	18	0	39.190
	26	18	18	25.060	0.270	18	18	25.330
	28	18	27	17.840	+ 0.280	18	27	18.120

Table XVIII.

To convert the Sun's observed Right Ascension, reduced by the Dublin Catalogue, into his correspondent Right Ascension reduced by the Greenwich Catalogue.

1821.

		Sun's R. A. observed at Dublin, the reductions being made by the Dublin Catalogue.	Equation for Diff. of Dublin and Greenwich Cat.	Sun's R. A. observed at Dublin, reduced by the Greenwich Catalogue.
		h. m. s.	s.	h. m. s.
June	30	6 36 8.500	+ 0.281	6 36 8.781
July	12	7 25 27.880	0.282	7 25 28.162
	19	7 53 45.250	0.298	7 53 45.548
August	23	10 8 17.650	0.207	10 8 17.857
	24	10 11 58.860	0.295	11 11 59.155
October	2	12 32 55.590	0.207	12 32 55.797
	29	14 13 51.050	0.261	14 13 51.311
December	6	16 51 7.100	0.291	16 51 7.391
	11	17 13 3.590	+ 0.288	17 13 3.878

Table XIX.

To convert the Sun's observed Right Ascension, reduced by the Dublin Catalogue, into his correspondent Right Ascension, by the Greenwich Catalogue.

1822.

		Sun's R. A. observed at Dublin, the reductions being made by the Dublin Catalogue.	Equation for Diff. of Dublin and Greenwich Cat.	Sun's R. A. observed at Dublin, reduced by the Greenwich Catalogue.
		h. m. s.	s.	h. m. s.
May	1	2 32 25.030	+ 0.272	2 32 25.302
	21	3 50 31.570	0.276	3 50 31.846
	22	3 54 31.920	0.289	3 54 32.209
	24	4 2 34.250	0.288	4 2 34.538
June	1	4 35 2.340	0.313	4 35 2.653
	3	4 43 13.840	0.303	4 43 14.143
	6	4 55 33.660	0.303	4 55 33.963
	7	4 59 40.680	0.307	4 59 40.987
	22	6 1 55.760	0.289	6 1 56.049
August	2	8 48 5.220	0.294	8 48 5.514
	19	9 52 37.390	0.273	9 52 37.663
	21	10 0 2.720	0.263	10 0 2.983
November	14	15 16 42.750	0.291	15 16 43.041
December	7	16 54 25.990	0.299	16 54 26.289
	26	18 18 31.830	+ 0.295	18 18 32.125

Table XX.

To show the Differences between the Sun's observed, and computed Right Ascensions; (by Greenwich observations).

1821.

		Sun's R. A. observed on the meridian of Greenwich.			Sun's R. A. computed for the meridian of Greenwich.			Difference of the observed and computed R. A.
		h.	m.	s.	h.	m.	s.	s.
June	30	6	36	4.28	6	36	3.50	+ 0.78
July	9	7	13	9.23	7	13	8.40	0.83
	12	7	25	23.69	7	25	23.00	0.69
	18	7	49	40.27	7	49	39.50	0.77
	19	7	53	41.08	7	53	40.50	0.58
August	3	8	52	48.87	8	52	48.20	0.67
	4	8	56	40.37	8	56	39.90	0.47
	10	9	19	38.07	9	19	37.30	0.77
	11	9	23	25.17	9	23	24.70	0.47
	17	9	45	58.73	9	45	57.90	0.83
	20	9	57	8.26	9	57	7.60	0.66
	21	10	0	50.94	10	0	50.00	0.94
	22	10	4	32.62	10	4	31.90	0.72
	23	10	8	13.75	10	8	13.30	0.45
	24	10	11	54.97	10	11	54.40	0.57
September	2	10	44	47.44	10	44	46.60	0.84
	3	10	48	24.69	10	48	24.00	0.69
	4	10	52	1.85	10	52	1.20	0.65
	5	10	55	38.45	10	55	38.10	0.35
	15	11	31	37.47	11	31	36.90	0.57
October	2	12	32	52.15	12	32	51.60	0.55
	21	13	43	7.39	13	43	6.50	0.89
	22	13	46	54.77	13	46	54.10	0.67
	29	14	13	47.11	14	13	46.70	0.41
	30	14	17	40.64	14	17	40.00	0.64
November	6	14	45	15.81	14	45	15.20	0.61
December	4	16	42	19.61	16	42	19.00	0.61
	5	16	46	40.49	16	46	40.40	0.09
	6	16	51	3.10	16	51	2.20	0.90
	8	16	59	47.87	16	59	47.50	0.37
	11	17	12	59.18	17	12	58.80	+ 0.38
					Mean by 31 obs. =			+ 0.627

Table XXI.

To show the Differences between the Sun's observed, and computed Right Ascensions; (by Greenwich observations).

1822.

		Sun's R. A. observed on the meridian of Greenwich.			Sun's R. A. computed for the meridian of Greenwich.			Difference of the observed and computed R. A.
		h.	m.	s.	h.	m.	s.	s.
January	15	19	47	12.68	19	47	12.50	+ 0.18
	16	19	51	30.44	19	51	30.00	0.44
February	21	22	17	21.84	22	17	21.50	0.34
	23	22	24	59.33	22	24	59.10	0.23
	24	22	28	47.00	22	28	47.00	0.00
	28	22	43	52.55	22	43	52.40	+ 0.15
March	1	22	47	37.20	22	47	37.30	− 0.10
April	30	2	28	32.50	2	28	32.20	+ 0.30
May	1	2	32	20.83	2	32	20.60	0.23
	21	3	50	27.49	3	50	26.70	0.79
	22	3	54	27.69	3	54	27.10	0.59
	24	4	2	30.05	4	2	29.50	0.55
	27	4	14	37.27	4	14	36.60	0.67
	31	4	30	52.93	4	30	52.60	0.33
June	1	4	34	58.18	4	34	57.70	0.48
	2	4	39	3.67	4	39	3.10	0.57
	3	4	43	9.43	4	43	9.00	0.43
	4	4	47	15.66	4	47	15.20	0.46
	6	4	55	29.28	4	55	28.60	0.68
	7	4	59	36.22	4	59	35.90	0.32
	22	6	1	51.47	6	1	50.90	0.57
July	7	7	3	57.50	7	3	56.80	0.70
	31	8	40	14.63	8	40	14.20	0.43
August	1	8	44	8.11	8	44	7.80	0.31
	2	8	48	1.27	8	48	0.70	0.57
	3	8	51	53.82	8	51	53.00	0.82
	4	8	55	45.40	8	55	44.80	0.60
	8	9	11	6.22	9	11	5.80	0.42
	17	9	45	6.16	9	45	5.70	0.46
	18	9	48	50.12	9	48	49.70	0.42
	19	9	52	33.35	9	52	33.20	0.15
	21	9	59	59.24	9	59	58.70	0.54
November	4	14	36	21.52	14	36	21.20	0.32
	9	14	56	19.00	14	56	19.00	0.00
	13	15	12	33.11	15	12	32.80	0.31
	14	15	16	38.72	15	16	38.40	0.32
	29	16	19	39.38	16	19	38.90	0.48
December	6	16	49	59.24	16	49	58.70	0.54
	7	16	54	21.47	16	54	21.00	0.47
	8	16	58	44.49	16	58	43.90	0.59
	22	18	0	41.17	18	0	40.80	0.37
	23	18	5	7.68	18	5	7.40	0.28
	26	18	18	27.31	18	18	26.90	0.41
	28	18	27	19.79	18	27	19.40	0.39
	30	18	36	11.80	18	36	11.00	+ 0.80

Mean by 45 obs. = + 0.420

Table XXII.

To show the Differences between the Sun's observed, and computed Right Ascensions. (By Paris Observations.)

1821.

		Sun's R. A. observed at Paris, reduced by the Greenwich Catalogue.	Sun's R. A. computed for the Meridian of Paris.	Difference of the observed and computed R. A.
		h. m. s.	h. m. s.	s.
July	18	7 49 38.210	7 49 37.935	+ 0.275
	19	7 53 39.470	7 53 38.938	0.532
August	20	9 57 6.830	9 57 6.156	0.674
	21	10 0 48.970	10 0 48.559	0.411
	22	10 4 31.150	10 4 30.462	0.688
	23	10 8 12.780	10 8 11.864	0.916
	24	10 11 53.790	10 11 52.968	0.822
September	2	10 44 46.000	10 44 45.188	0.812
	4	10 52 0.440	10 51 59.792	0.648
	5	10 55 37.360	10 55 36.693	0.667
October	2	12 32 50.600	12 32 50.185	0.415
	21	13 43 5.940	13 43 5.022	0.918
	29	14 13 45.630	14 13 45.185	0.445
	30	14 17 38.810	14 17 38.480	0.330
November	6	14 45 14.010	14 45 13.644	0.366
December	4	16 42 17.720	16 42 17.303	+ 0.417
			Mean by 16 obs. =	+ 0.584

Table XXIII.

To show the Differences between the Sun's observed, and computed Right Ascensions. (By Paris Observations.)

1822.

		Sun's R. A. observed at Paris, reduced by the Greenwich Catalogue.			Sun's R. A. computed for the Meridian of Paris.			Difference of the observed and computed R. A.
		h.	m.	s.	h.	m.	s.	s.
January	15	19	47	10.900	19	47	10.828	+ 0.072
February	21	22	17	20.650	22	17	20.013	0.637
	23	22	24	57.990	22	24	57.620	0.370
	28	22	43	51.550	22	43	50.940	0.610
March	1	22	47	36.280	22	47	35.844	0.436
April	30	2	28	30.890	2	28	30.717	0.173
May	1	2	32	19.580	2	32	19.114	0.466
	21	3	50	25.630	3	50	25.140	0.490
	22	3	54	26.620	3	54	25.536	1.084
	31	4	30	51.780	4	30	51.009	0.771
June	1	4	34	56.340	4	34	56.107	0.233
	2	4	39	2.130	4	39	1.503	0.627
	3	4	43	8.220	4	43	7.401	0.819
	4	4	47	14.230	4	47	13.599	0.631
	6	4	55	27.600	4	55	26.994	0.606
	7	4	59	35.430	4	59	34.293	1.137
July	4	6	51	35.200	6	51	34.595	0.605
	10	7	16	13.890	7	16	12.809	1.081
August	1	8	44	6.260	8	44	6.288	0.972
	2	8	48	0.060	8	47	59.192	0.868
	17	9	45	4.630	9	45	4.246	0.384
	18	9	48	48.600	9	48	48.249	0.351
November	26	16	6	47.850	16	6	47.438	0.412
December	6	16	49	57.380	16	49	56.997	0.383
	7	16	54	19.960	16	54	19.293	0.667
	22	18	0	39.190	18	0	39.069	0.121
	26	18	18	25.330	18	18	25.171	0.159
	28	18	27	18.120	18	27	17.673	+ 0.447
							Mean by 28 obs. =	+ 0.558

Table XXIV.

To show the Differences between the Sun's observed, and computed Right Ascensions; (by Dublin observations.)

1821.

		Sun's R. A. observed at Dublin, reduced by the Greenwich Catalogue.			Sun's R. A. computed for the meridian of Dublin Observatory.			Diff. of the observed and computed R. A.
		h.	m.	s.	h.	m.	s.	s.
June	30	6	36	8.781	6	36	7.842	+ 0.939
July	12	7	25	28.162	7	25	27.298	0.864
	19	7	53	45.548	7	53	44.737	0.811
August	23	10	8	17.857	10	8	17.195	0.662
	24	11	11	59.155	10	11	58.286	0.869
October	2	12	32	55.797	12	32	55.440	0.357
	29	14	13	51.311	14	13	50.810	0.501
December	6	16	51	7.391	16	51	6.822	0.569
	11	17	13	3.878	17	13	3.459	+ 0.419
							Mean by 9 obs. =	+ 0.666

Table XXV.

To show the Differences between the Sun's observed, and computed Right Ascensions; (by Dublin observations.)

1822.

		Sun's R. A. observed at Dublin, reduced by the Greenwich Catalogue.			Sun's R. A. computed for the meridian of Dublin Observatory.			Diff. of the observed aud computed R. A.
		h.	m.	s.	h.	m.	s.	s.
May	1	2	32	25.302	2	32	24.630	+ 0.672
	21	3	50	31.846	3	50	30.933	0.913
	22	3	54	32.209	3	54	31.344	0.865
	24	4	2	34.538	4	2	33.761	0.777
June	1	4	35	2.653	4	35	2.023	0.630
	3	4	43	14.143	4	43	13.337	0.806
	6	4	55	33.963	4	55	32.956	1.007
	7	4	59	40.987	4	59	40.260	0.727
	22	6	1	56.049	6	1	55.297	0.752
August	2	8	48	5.514	8	48	4.792	0.722
	19	9	52	37.663	9	52	37.128	0.535
	21	10	0	2.983	10	0	2.611	0.372
November	14	15	16	43.041	15	16	42.740	0.301
December	7	16	54	26.289	16	54	25.631	0.658
	26	18	18	32.125	18	18	31.573	+ 0.552
							Mean by 15 obs. =	+ 0.686

Sloane Street,
May 24, 1826.

JAMES SOUTH.

The following was received from Dr. Young whilst the preceding Memoir was in the press.

JAMES SOUTH.

Park Square, 10th July, 1826.

Dear Sir,

I send you some computations of the Sun's longitude from the observations made at Greenwich in 1820, compared with Delambre's Tables, as corrected by Burckhardt and Bouvard, and with Carlini's, as modified by some slight corrections communicated by Professor Schumacher. The calculations have been made at the expense of the Board of Longitude; and if you think they would tend to illustrate the subject of your Paper, you will perhaps have no objection to inserting them as a note at the end. I have had the same observations reduced by an able astronomer in Germany; but the results are not immediately comparable with these, as they show the errors in right ascension only; and they make the error of Carlini's tables rather greater than is here represented, amounting to about — 8″ on an average, instead of — 3″ or + 1″; so that the mode of reduction appears to require some further examination.

I am, Dear Sir,
Your faithful and obedient Servant,
Thomas Young, M. D.
Sec. Bd. Long.

James South, Esq. &c. &c. &c.

Date.	The Sun's Longitude, by												Error of Delambre's Tables improved.	Error of Carlini's Tables.	Error of Carlini's Tables improved.
	Observation.			Delambre's Tables improved.			Carlini's Tables.			Carlini's Tables improved.					
1820.	°	′	″	°	′	″	°	′	″	°	′	″	″	″	″
Jan. 1	280	3	48,2	280	3	52,9	280	3	47,2	280	3	51,2	+ 4,7	— 1,0	+ 3,0
3	282	6	7,9	282	6	12,8	282	6	6,8	282	6	11,9	+ 4,9	— 1,1	+ 4,0
5	284	8	27,3	284	8	33,2	284	8	26,8	284	8	32,0	+ 5,9	— 0,5	+ 4,7
15	294	20	8,0	294	20	12,8	294	20	6,9	294	20	11,6	+ 4,8	— 1,1	+ 3,6
16	295	21	20,0	295	21	21,2	295	21	15,7	295	21	20,1	+ 1,2	— 4,3	+ 0,1
22	301	27	51,9	301	27	54,6	301	27	49,0	301	27	52,9	+ ,7	— 2,9	+ 1,0
23	302	28	52,2	302	28	56,6	302	28	51,1	302	28	55,0	+ 4,4	— 1,1	+ 2.8
27	306	32	47,4	306	32	52,2	306	32	46,7	306	32	50,7	+ 4,8	— 0,7	+ 3,3
31	310	36	23,7	310	36	29,8	310	36	23,6	310	36	28,0	+ 6,1	— 0,1	+ 4,3

Date.	The Sun's Longitude, by				Error of Delambre's Tables improved.	Error of Carlini's Tables.	Error of Carlini's Tables improved.
	Observation.	Delambre's Tables improved.	Carlini's Tables.	Carlini's Tables improved.			
1820.	° ′ ″	° ′ ″	° ′ ″	° ′ ″	″	″	″
Feb. 1	311 37 16,0	311 37 21,6	311 37 15,3	311 37 19,8	+ 5,6	— 0,7	+ 3,8
14	324 46 54,9	324 46 57,8	324 46 53,3	324 46 57,0	+ 2,9	— 1,6	+ 2,1
15	325 47 25,0	325 47 33,4	325 47 29,0	325 47 32,6	+ 8,4	+ 4,0	+ 7,6
16	326 48 5,3	326 48 7,5	326 48 3,1	326 48 6,6	+ 2,2	— 2,2	+ 1,3
17	327 48 38,8	327 48 39,8	327 48 35,3	327 48 38,7	+ 1,0	— 3,5	— 0,1
21	331 50 27,0	331 50 29,3	331 50 25,3	331 50 28,5	+ 2,3	— 1,7	+ 1,5
27	337 52 16,0	337 52 14,7	337 52 10,5	337 52 13,9	— 1,3	— 5,5	— 2,1
28	338 52 25,3	338 52 25,7	338 52 21,4	338 52 24,8	+ 0,4	— 3,9	— 0,5
29	339 52 35,3	339 52 34,5	339 52 30,3	339 52 33,7	— 0,8	— 5,0	— 1,6
March 8	347 52 39,5	347 52 43,5	347 52 39,8	347 52 43,5	+ 4,0	+ 0,3	+ 4,0
9	348 52 39,3	348 52 37,1	348 52 33,3	348 52 36,8	— 2,2	— 6,0	— 2,5
10	349 52 28,7	349 52 29,3	349 52 26,0	349 52 29,5	+ 0,6	— 2,7	+ 0,8
11	350 52 23,3	350 52 19,5	350 52 16,4	350 52 19,8	— 3,8	— 6,9	— 3,5
13	352 51 57,8	352 51 54,4	352 51 52,0	352 51 55,2	— 3,4	— 5,8	— 2,6
15	354 51 24,9	354 51 21,9	354 51 19,5	354 51 22,4	— 3,0	— 5,4	— 2,5
23	2 47 48,5	2 47 44,2	2 47 42,5	2 47 44,7	— 4,3	— 6,0	— 3,8
30	9 42 34,9	9 42 34,2	9 42 31,6	9 42 34,3	— 0,7	— 3,3	— 0,6
April 5	15 36 52,2	15 36 49,4	15 36 47,5	15 36 50,5	— 2,8	— 4,7	— 1,7
7	17 34 36,0	17 34 40,4	17 34 38,7	17 34 41,7	+ 4,4	+ 2,7	+ 5,7
11	21 30 3,7	21 30 1,6	21 30 0,4	21 30 3,0	— 2,1	— 3,3	— 0,7
15	25 24 55,7	25 24 53,1	25 24 52,1	25 24 54,3	— 2,6	— 3,6	— 1,4
17	27 22 6,4	27 22 6,6	27 22 6,1	27 22 8,0	+ 0,2	— 0,3	+ 1,6
18	28 20 42,8	28 20 40,0	28 20 39,4	28 20 41,2	— 2,8	— 3,4	— 1,6
19	29 19 14,1	29 19 11,3	29 19 10,8	29 19 12,6	— 2,8	— 3,3	— 1,5
21	31 16 10,5	31 16 7,3	31 16 6,6	31 16 8,4	— 3,2	— 3,9	— 2,1
22	32 14 31,4	32 14 32,1	32 14 31,3	32 14 33,1	+ 0,7	— 0,1	+ 1,7
23	33 12 55,7	33 12 54,8	33 12 53,9	33 12 55,7	— 0,9	— 1,8	0,0
24	34 11 15,4	34 11 15,3	34 11 14,3	34 11 16,2	— 0,1	— 1,1	+ 0,8
25	35 9 38,8	35 9 33,8	35 9 32,7	35 9 34,7	— 5,0	— 6,1	— 4,1
26	36 7 53,7	36 7 50,6	36 7 49,0	36 7 51,1	— 3,1	— 4,7	— 2,6
28	38 4 21,1	38 4 18,4	38 4 16,6	38 4 19,0	— 2,7	— 4,5	— 2,1
29	39 2 34,3	39 2 29,6	39 2 27,6	39 2 30,1	— 4,7	— 6,7	— 4,2
May 5	44 51 3,5	44 51 4,1	44 51 2,6	44 51 5,3	+ 0,6	— 0,9	+ 1,8
7	46 47 9,4	46 47 5,0	46 47 3,4	46 47 6,1	— 4,4	— 6,0	— 3,3
12	51 36 46,7	51 36 44,4	51 36 43,0	51 36 45,2	— 2,3	— 3,7	— 1,5
15	54 30 16,7	54 30 14,9	54 30 13,9	54 30 15,8	— 1,8	— 2,8	— 0,9
21	60 16 33,5	60 16 33,5	60 16 32,6	60 16 34,4	0,0	— 0,9	+ 0,9
22	61 14 9,9	61 14 11,2	61 14 10,0	61 14 11,9	+ 1,3	+ 0,1	+ 2,0
23	62 11 47,8	62 11 47,3	62 11 46,0	62 11 48,0	— 0,5	— 1,8	+ 0,2
24	63 9 23,5	63 9 21,9	63 9 20,7	63 9 22,8	— 1,6	— 2,8	— 0,7
June 1	70 49 18,5	70 49 18,4	70 49 16,0	70 49 18,0	— 0,1	— 2,5	+ 0,3
17	86 7 0,9	86 6 59,3	86 6 57,3	86 6 59,6	— 1,6	— 3,6	— 1,3
23	91 50 20,9	91 50 22,2	91 50 19,3	91 50 22,0	+ 1,3	— 1,6	+ 1,1
24	92 47 33,9	92 47 33,9	92 47 30,8	92 47 33,7	0,0	— 3,1	— 0,2
25	93 44 50,1	93 44 45,4	93 44 42,0	93 44 44,9	— 4,7	— 8,1	— 5,2
26	94 41 56,2	94 41 56,5	94 41 53,0	94 41 56,0	+ 0,3	— 3,2	— 0,2
27	95 39 6,6	95 39 7,3	95 39 3,8	95 39 7,0	+ 0,7	— 2,8	+ 0,4
28	96 36 17,0	96 36 18,1	96 36 14,8	96 36 18,2	+ 1,1	— 2,2	+ 1,2
29	97 33 27,4	97 33 29,1	97 33 25,8	97 33 29,3	+ 1,7	— 1,6	+ 1,9

Date.		The Sun's Longitude, by												Error of Delambre's Tables improved.	Error of Carlini's Tables.	Error of Carlini's Tables improved.
		Observation.			Delambre's Tables improved			Carlini's Tables.			Carlini's Tables improved.					
1820.		°	′	″	°	′	″	°	′	″	°	′	″	″	″	″
July	10	108	2	47,2	108	2	49,8	108	2	45,8	108	2	49,2	+ 2,6	— 1,4	+ 2,0
	11	109	0	2,6	109	0	4,3	109	0	0,6	109	0	3,9	+ 1,7	— 2,0	+ 1,3
	19	116	38	7,1	116	38	8,6	116	38	4,8	116	38	8,1	+ 1,5	— 2,3	+ 1,0
	24	121	24	33,5	121	24	34,8	121	24	30,1	121	24	33,7	+ 1,3	— 3,4	+ 0,2
	27	124	16	34,8	124	16	32,4	124	16	28,1	124	16	32,2	— 2,4	— 6,7	— 2,6
	30	127	8	40,4	127	8	39,2	127	8	34,7	127	8	38,9	— 1,2	— 5,7	— 1,5
Aug.	1	129	3	28,4	129	3	28,9	129	3	24,4	129	3	29,0	+ 0,5	— 4,0	+ 0,6
	7	134	48	29,5	134	48	29,9	134	48	24,9	134	48	29,4	+ 0,4	— 4,6	— 0,1
	8	135	46	4,0	135	46	4,3	135	45	59,4	135	46	3,8	+ 0,3	— 4,6	— 0,2
	9	136	43	40,1	136	43	39,8	136	43	35,1	136	43	39,4	— 0,3	— 5,0	— 0,7
	10	137	41	16,1	137	41	16,8	137	41	11,9	137	41	16,1	+ 0,7	— 4,2	0,0
	11	138	38	54,3	138	38	54,6	138	38	49,8	138	38	53,9	+ 0,3	— 4,5	— 0,4
	12	139	36	34,7	139	36	33,4	139	36	28,7	139	36	32,8	— 1,3	— 6,0	— 1,9
	13	140	34	9,5	140	34	13,1	140	34	8,7	140	34	12,7	+ 3,6	— 0,8	+ 3,2
	14	141	31	54,0	141	31	53,7	141	31	49,6	141	31	53,6	— 0,3	— 4,4	— 0,4
	18	145	22	46,9	145	22	48,4	145	22	43,6	145	22	47,6	+ 1,5	— 3,3	+ 0,7
	19	146	20	35,1	146	20	34,6	146	20	29,7	146	20	33,8	— 0,5	— 5 4	— 1,3
	20	147	18	21,5	147	18	21,6	147	18	17,0	147	18	21,3	+ 0,1	— 4 5	— 0,2
	27	154	3	35,5	154	3	31,9	154	3	27,3	154	3	32,3	— 3,6	— 8,2	— 3,2
	29	155	59	37,0	155	59	32,0	155	59	27,2	155	59	32,5	— 5,0	— 9 8	— 4,5
	30	156	57	41,3	156	57	36,4	156	57	31,4	156	57	36,7	— 4,9	— 9,9	— 4,6
Sept.	1	158	53	46,2	158	53	49,4	158	53	44,0	158	53	49,3	+ 3,2	— 2,2	+ 3,1
	2	159	52	0,3	159	51	58,9	159	51	53,6	159	51	58,8	— 1,4	— 6,7	— 1,5
	3	160	50	14,0	160	50	10,5	160	50	4,9	160	50	10,1	— 3,5	— 9,1	— 3,9
	4	161	48	22,7	161	48	23,6	161	48	18,2	161	48	23,4	+ 0,9	— 4,5	+ 0,7
	5	162	46	39,0	162	46	38,8	162	46	33,5	162	46	38,6	— 0,2	— 5,5	— 0,4
	6	163	44	51,0	163	44	55,7	163	44	50,6	163	44	55,6	+ 4,7	— 0,4	+ 4 6
	7	164	43	14,3	164	43	15,1	164	43	9,9	164	43	14,8	+ 0,8	— 4,4	+ 0,5
	8	165	41	32,5	165	41	35,7	165	41	30,7	165	41	35,5	+ 3,2	— 1,8	+ 3,0
	9	166	39	55,1	166	39	58,2	166	39	53,7	166	39	58,5	+ 3,1	— 1,4	+ 3,4
	10	167	38	22,6	167	38	22,5	167	38	18,1	167	38	22,8	— 0,1	— 4,5	+ 0,2
	11	168	36	42,0	168	36	48,3	168	36	44,1	168	36	48,7	+ 6,3	+ 2,1	+ 6,7
	13	170	33	43,9	170	33	45,3	170	33	41,4	170	33	45,9	+ 1,4	— 2,5	+ 2,0
	19	176	25	14,2	176	25	15,8	176	25	11,8	176	25	16,5	+ 1,6	— 2,4	+ 2,3
	24	181	18	58,0	181	18	59,9	181	18	55,7	181	19	0,9	+ 1,9	— 2,3	+ 2,9
Oct.	3	190	9	58,1	190	10	2,5	190	9	57,1	190	10	2,6	+ 4,4	— 1,0	+ 4,5
	5	192	8	25,4	192	8	28,3	192	8	23,0	192	8	28,3	+ 2,9	— 2,4	+ 2,9
	6	193	7	41,2	193	7	44,5	193	7	39,5	193	7	44,7	+ 3,3	— 1,7	+ 3,5
	12	199	3	59,1	199	4	3,5	199	3	59,7	199	4	4,4	+ 4,4	+ 0,6	+ 5,3
	17	204	1	51,1	204	1	50,5	204	1	46,4	204	1	51,0	— 0,6	— 4,7	— 0,1
	28	214	59	42,4	214	59	43,4	214	59	38,4	214	59	43,7	+ 1,0	— 4,0	+ 1,3
Nov.	2	220	0	8,0	220	0	11,4	220	0	6,5	220	0	11,6	+ 3,4	— 1,5	+ 3,6
	3	221	0	22,1	221	0	23,4	221	0	18,7	221	0	23,7	+ 1,3	— 3,4	+ 1,6
	16	234	5	28,7	234	5	32,2	234	5	28,3	234	5	32,4	+ 3,5	— 0,4	+ 3,7
	18	236	6	35,6	236	6	40,4	236	6	36,8	236	6	41,1	+ 4,8	+ 1,2	+ 5,5
	19	237	7	10,3	237	7	16,7	237	7	13,1	237	7	17,5	+ 6,4	+ 2,8	+ 7,2
	27	245	12	51,7	245	13	1,0	245	12	56,4	245	13	1,2	+ 9,3	+ 4,7	+ 9,5
Dec.	14	262	29	41,5	262	29	47,0	262	29	43,3	262	29	46,6	+ 5,5	+ 1,8	+ 5,1
	28	276	45	34,6	276	45	40,6	276	45	35,7	276	45	39,5	+ 6,0	+ 1,1	+ 4,9
														+ 177,0 — 94,6	+ 21,4 — 335,6	+ 164,5 — 83,6
														± 271,6	± 357,0	± 248,1
														+ 82,4	— 314,2	+ 80,9

XXIX. *On the existence of a limit to vaporization.* *By* M. Faraday, *F. R. S. Corresponding Member of the Royal Academy of Sciences at Paris, &c. &c.* *Communicated May 26, 1826.*

Read June 15, 1826.

It is well known that within the limits recognised by experiment, the constitution of vapour[*] in contact with the body from which it rises, is such, that its tension increases with increased temperature, and diminishes with diminished temperature; and, though in the latter case we can, with many substances, so far attenuate the vapour as soon to make its presence inappreciable to our tests, yet an opinion is very prevalent, and I believe general,[†] that still small portions are produced; the tension being correspondent to the comparatively low temperature of the substance. Upon this view, it has been supposed that every substance in vacuo or surrounded by vapour or gas, having no chemical action upon it, has an atmosphere of its own around it; and that our atmosphere must contain, diffused through it, minute portions of the vapours of all those substances with which it is in contact, even down to the earths and metals. I believe that a theory of meteorites has been formed upon this opinion.

Perhaps the point has never been distinctly considered;

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[*] By the term vapour, I mean throughout this paper that state of a body in which it is permanently and indefinitely elastic.

[†] See Sir H. Davy's paper on Electrical Phenomena exhibited in Vacuo. Phil. Trans. MDCCCXXII. p. 70.

and it may therefore not be uninteresting to urge two or three reasons, in part dependant upon experimental proof, why this should not be the case. The object, therefore, which I shall hold in view in the following pages, is to show that a *limit* exists to the production of vapour of any tension by bodies placed in vacuo, or in elastic media, beneath which limit they are perfectly fixed.

Dr. Wollaston, by a beautiful train of argument and observation, has gone far to prove that our atmosphere is of finite extent, its boundary being dependant upon the opposing powers of elasticity and gravitation.[*] On passing upwards, from the earth's surface, the air becomes more and more attenuated, in consequence of the gradually diminishing pressure of the superincumbent part, and its tension or elasticity is proportionally diminished; when the diminution is such, that the elasticity is a force, not more powerful than the attraction of gravity, then a limit to the atmosphere must occur. The particles of the atmosphere there tend to separate with a certain force; but this force is not greater than the attraction of gravity, which tends to make them approach the earth and each other; and as expansion would necessarily give rise to diminished tension, the force of gravity would then be strongest, and consequently would cause contraction, until the powers were balanced as before.

Assuming this state of things as proved, the air at the limit of the atmosphere has a certain degree of elasticity or tension; and, although it cannot there exist of smaller tension, yet, if portions of it were removed to a farther distance from the earth, or if the force of gravity over it could in any other way be diminished, then it would expand, and exist of a lower tension; upon the renewal of the gravitating force, either by approximation to the earth's surface or otherwise, the particles would approach each other, until the elasticity of the whole was again equal to the force of gravity.

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[*] Phil. Trans. MDCCCXXII. p. 89.

Inasmuch as gases and vapours undergo no change by mere expansion or attenuation, which can at all disturb the analogy existing between them in their permanent state under ordinary circumstances, all the phenomena which have been assumed as occurring with the air at the limit of our atmosphere may, with equal propriety, be admitted with respect to vapour in general in similar circumstances; for we have no reason for supposing that the particles of one vapour more than another are *free* from the influence of gravity, although the force may, and without doubt does, vary, with the weight and elasticity of the particles of each particular substance.

It will be evident, also, that similar effects would be produced by the force of gravity upon air or vapour of the extreme tenuity and feeble tension referred to, whatever be the means taken to bring it into that state; and it is not necessary to imagine the portion of air operated upon, as taken from the extremity of our atmosphere, for a portion of that at the earth's surface, if it could be expanded to the same degree by an air pump, would undergo the same changes: when of a certain rarity it would just balance the attraction of gravitation and fill the receiver with vapour; but then, if half were taken out of the receiver, the remaining portion, in place of filling the vessel, would submit to the force of gravity, would contract into the lower half of the receiver, until, by the approximation of their particles, the vapour there existing should have an elasticity equal to the force of gravity to which it was subject. This is a necessary consequence of Dr. WOLLASTON's argument.

There is yet another method of diminishing the elasticity of vapour, namely, by diminution of temperature. With respect to the most elastic substances, as air, and many gases, the comparatively small range which we can command beneath common temperatures, does nothing more at the earth's surface than diminish in a slight degree their elasticity, though two or three of them, as sulphurous acid and chlorine, have been in part condensed into liquids. But with respect to innumerable bodies, their tendency to form vapour is so small, that at common temperatures the vapour produced approximates in rarity to the air upon the limits of our atmosphere; and with these, the power we possess of lessening tension by diminution of temperature, may be quite sufficient to render it a smaller force than its opponent, gravity; in which case it will be easy to comprehend that the vapour would give way to the latter, and be entirely condensed. The metal, silver, for instance, when violently heated, as on charcoal urged by a jet of oxygen, or by the oxy-hydrogen, or oxy-alcohol flame, is converted into vapour; lower the temperature, and before the metal falls beneath a white heat, the tension of the vapour is so far diminished, that its existence becomes inappreciable by the most delicate tests. Suppose, however, that portions are formed, and that vapour of a certain tension is produced at that temperature; it must be astonishingly diminished by the time the metal has sunk to a mere red heat; and we can hardly conceive it possible, I think, that the silver should have descended to common temperatures, before its accompanying vapour will, by its gradual diminution in tension, if uninfluenced by other circumstances, have had an elastic force far inferior to the force of gravity; in which case, that moment at which the two forces had become equal, would be the last moment in which vapour could exist around it; the metal at every lower temperature being perfectly fixed.

I have illustrated this case by silver, because, from the high temperature required to make any vapour appreciable, there can be little doubt, that the equality of the gravitating and elastic forces, must take place much above common temperatures, and therefore within the range which we can command. But there is, I think, reason to believe that the equality in these forces, at or above ordinary temperatures, may take place with bodies far more volatile than silver; with substances indeed which boil under common circumstances at 600° or 700° F.

If, as I have formerly shown,* some clean mercury be put at the bottom of a clean dry bottle, a piece of gold leaf attached to the under part of the stopper by which it is closed, and the whole left for some months at a temperature of from 60° to 80°, the gold leaf will be found whitened by amalgamation, in consequence of the vapour which rises from the mercury beneath; but upon making the experiment in the winter of 1824-5, I was unable to obtain the effect, however near the gold leaf was brought to the surface of the mercury; and I am now inclined to believe, because the elastic force of any vapour which the mercury could have produced at that temperature, was less than the force of gravity upon it, and that consequently the mercury was then perfectly fixed.

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* Quarterly Journal of Science, X. 354.

Sir HUMPHRY DAVY, in his experiments on the electrical phenomena exhibited in vacuo, found, that when the temperature of the vacuum above mercury was lowered to 20° F. no further diminution, even down to —20° F. was able to effect any change, as to the power of transmitting electricity, or in the luminous appearances; and that these phenomena were then nearly of the same intensity as in the vacuum made over tin.* Hence, in conjunction with the preceding reasoning, I am led to conclude, that they were then produced independant of any vapour of the metals, and that under the circumstances described; no vapour of mercury existed at temperatures beneath 20° F.

Concentrated sulphuric acid boils at about 600° F. but as the temperature is lowered the tension of its vapour is rapidly diminished. Signor BELLANI † placed a thin plate of zinc at the upper part of a closed bottle, at the bottom of which was some concentrated sulphuric acid. No action had taken place at the end of two years, the zinc then remaining as bright as at first; and this fact is very properly adduced in illustration of the fixedness of sulphuric acid at common temperatures. Here I should again presume, that the elastic force which tended to form vapour, was surpassed by the force of gravity.

Whether it be admitted or not, that in these experiments the limit of volatilization, according to the principle of the balance of forces before stated had been obtained, I think, we can hardly doubt that such is the case at common temperatures, with respect to the silver, and with all bodies which bear a high temperature without appreciable loss by volatilization, as platina, gold, iron, nickel, silica, alumina, charcoal, &c.; and, consequently that, at common temperatures, no portion of vapour rises from these bodies or surrounds them; that they are really and truly fixed; and that none of them can exist in the atmosphere in the state of vapour.

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* Phil. Trans. MDCCCXXII. p. 71.

† Gionale di Fisica, v. 197.

But there is another force, independent of that of gravity, at least of the general gravity of the earth, which appears to me sufficient to overcome a certain degree of vaporous elasticity, and, consequently, competent to the condensation of vapour of inferior tension, even though gravity should be suspended; I mean the force of homogeneous attraction.

Into a clean glass tube, about half an inch in diameter, introduce a piece of camphor; contract the tube at the lamp about four inches from the extremity; then exhaust it, and seal it hermetically at the contracted part; collect the camphor to one end of the tube; and then, having placed the tube in a convenient position, cool the other end slightly, as by covering it with a piece of bibulous paper preserved in a moist state by a basin of water and thread of cotton; in this way, a difference in temperature of a few degrees will be occasioned between the ends of the tube, and after some days, or a week or two, crystals of camphor will be deposited in the cooled part; there will not, however, be more than three or four of them, and these will continue to increase in size as long as the experiment is undisturbed, without the formation of any new crystals, unless the difference of temperature be considerable.

A little consideration will, I think, satisfy us that, after the first formation of the crystals in the cooled part, they have the power of diminishing the tension of the vapour of camphor, below that point at which it could have remained unchanged in contact with the glass, or in space: for the vapour of the camphor is of a certain tension in the cooled end of the tube, which it can retain in contact with the glass, and therefore it remains unchanged; but which it cannot retain in contact with the crystal of camphor, for there it is condensed, and continually adds to its mass. Now, this can only be in consequence of a positive power in the crystal of camphor of attracting other particles to it; and the phenomena of the experiment are such as to show, that the force is able to overcome a certain degree of elasticity in the surrounding vapour. There is therefore no difficulty in conceiving, that, by diminishing the temperature of a body and its atmosphere of vapour, the tension of the latter may be so far decreased, as at last to be inferior to the force with which the solid portion, by the attraction of aggregation, draws the particles to it; in which case it would immediately cause the entire condensation of the vapour.

The preceding experiment may be made with iodine, and many other substances; and indeed there is no case of distinct crystallization by sublimation* which does not equally afford evidence of the power of the solid matter, to overcome a positive degree of tension in the vapour from which the crystals are formed. The same power, or the force of aggregation, is also illustrated in crystallizing solutions; where the solution has a tendency to deposit upon a crystal, when it has not the same tendency to deposit elsewhere.

It may be imagined that crystallization would scarcely go on from these attenuated vapours, as it does in the denser states of the vapours experimented upon. There is, however, no good reason for supposing any difference in the force of aggregation of a solid body, dependent upon changes in the tension of the vapour about it; and indeed, generally speaking, the method I have assumed for diminishing the tension of the vapour, namely, by diminishing temperature, would cause increase in the force of aggregation.

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* Calomel, corrosive sublimate, oxide of antimony, naphthaline, oxalic acid, &c. &c.

Such are the principal reasons which have induced me to believe in the existence of a limit to the tension of vapour. If I am correct, then there are at least two causes, each of which is sufficient to overcome and destroy vapour when reduced to a certain tension; and both of which are acting effectually with numerous substances upon the surface of the earth, and retaining them in a state of perfect fixity. I have given reasons for supposing that the two bodies named, which boil at about 600° F. are perfectly fixed within limits of low temperature which we can command; and I have no doubt, that, nearly all the present recognised metals, the earths, carbon, and many of the metallic oxides, besides the greater number of their compounds, are perfectly fixed bodies at common temperatures. The smell emitted by various metals when rubbed may be objected to these conclusions, but the circumstances under which these odours are produced, are such, as not to leave any serious objections on my mind to the opinions above advanced.

I refrain from extending these views, as might easily be done, to the atomic theory, being rather desirous that they should first obtain the sanction or correction of scientific men. I should have been glad to have quoted more experiments upon the subject, and especially relative to such bodies as acquire their fixed point at, or somewhat below common temperatures. Captain FRANKLIN has kindly undertaken to make certain experiments for me in the cold regions to which he has gone, and probably when he returns from his arduous undertaking, he may have some contributions towards this subject.

Royal Institution,
May 4, 1826.

XXX. *On electrical and magnetic rotations.* By CHARLES BABBAGE, *Esq. F.R.S. &c. &c. Communicated May* 29, 1826.

Read June 15, 1826.

THE known fact, that artificial magnets receive additional increments to their power by slowly adding weight to the load they support, combined with the circumstance, that pieces of iron or steel exposed to the influence of a magnet, neither acquire nor lose their magnetism instantaneously, led me to explain Mr. BARLOW'S experiments "On the temporary magnetic effect induced in iron bodies by rotation,"* without having recourse to any new property of matter, and to apply the same consideration of the time of acquiring and losing magnetic virtue to the curious experiments of M. ARAGO. These views are stated in a paper printed in the Phil. Trans. for 1825, which contains the observations that occurred to Mr. HERSCHEL and myself, in repeating and varying those experiments.

As the explanation there given is intimately connected with the experiments detailed in the present paper, and as they were undertaken with a view of giving additional evidence to the principle to which I have alluded, I shall here briefly re-state the manner in which time influences the results of certain magnetic phenomena, whose production depends on the application of motion to some part of the apparatus employed.

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[*] Phil. Trans. 1825.

Fig. 1.

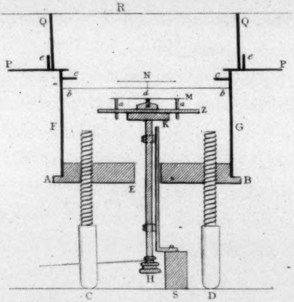


Fig. 2.

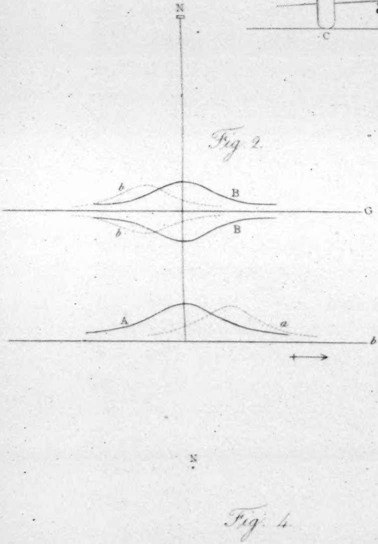


Fig. 3.

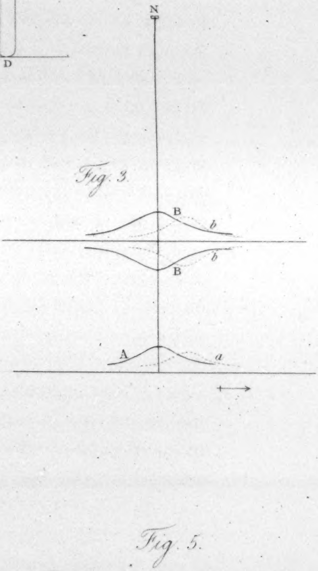


Fig. 4.

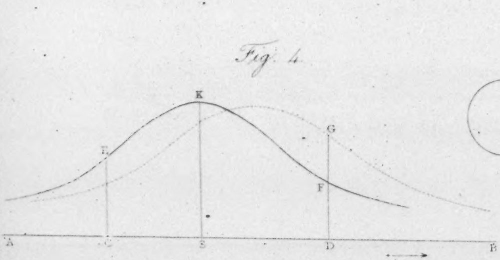
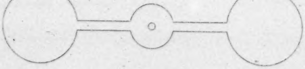


Fig. 5.



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If N represent the north pole of a magnet, Plate XIX, fig. 4, placed above an indefinite plate AB, of iron or any substance susceptible of magnetism, then the point S immediately under N will become a south pole from induced magnetism: but the two poles will attract each other, and if they are prevented from moving directly towards each other, motion cannot arise in any other direction. The point S immediately under N will possess the strongest south polarity, but other adjacent points will also acquire the same property in a smaller degree: if an ordinate SK be erected at S, and if at every other point in the line AB other ordinates be raised, each proportional to the intensity of the induced magnetism at that point, a curve EKF will be formed, which will represent the magnetic force of the plate, and at equal distances CS, SD, on each side of the point S the ordinates EC and FD will be equal.

If the plate is now supposed to be moved in the direction AB, and if the time in which it acquires or gives up any portion of magnetism be not instantaneous, then the ordinates which represent the magnetic intensities at equal distances from the middle point S will not be equal: for the point D has, in consequence of the motion of the plate, just passed through a point S, considerably nearer to the source of magnetism, at which point its magnetic intensity was expressed by SK. As it is supposed not to part immediately with its acquired magnetism, some portion will remain when it reaches the point D: therefore the ordinate DG, at this point, will be greater than it is when the plate is at rest; consequently the curve EKG, expressing the magnetic force when the plate is in motion, will, on the side towards which the motion is directed, be exterior to that which denotes the same force when the plate is at rest. But in the case of the plate being in motion, the force which is measured by the area of the curve is greater on one side than on the other; there will therefore be a small tendency in the plate to draw the point N towards that side to which it is moving. The points which have not passed vertically under the pole N, will in reality be a little less magnetised than they would be if they were at rest, because when they reach any particular place, they may not be exposed to the magnetic influence for a time sufficient for them to acquire the full degree of magnetism which they would obtain, if they remained longer in that place. If the moving plate be circular, and if the magnet be suspended in a balance of torsion, this extremely small tendency of each part to drag the pole N in the direction of its motion being repeatedly applied by each succeeding particle, and during many revolutions, may at last produce motion in the magnets above, even though the attractive force of the revolving substance be so exceedingly feeble as to be insensible to other methods.

In this analysis of the action of a magnet on a moving body susceptible of magnetism, it is to be observed, that no part of the effect is attributed to the velocity with which the agent* immediately producing the attraction traverses from the former to the latter: the nature of the action is not therefore analogous to that which produces the aberration of light.

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* It is difficult to write on any physical subject without employing words which are more or less connected with the various hypotheses prevailing at the time. In the present Paper I have no intention of giving any opinion on the theories at present received, but merely propose stating facts that I have noticed, and the reasonings to which they appear to me to lead.

The essential circumstance in producing the rotation of the suspended magnet is, that the substance revolving below it shall acquire and lose its magnetism in a finite time, and not instantly. It appeared, therefore, that if any other attractive or repulsive property exists in matter which is capable of communication to other matter situated near it; and if such communication require time for its performance; and if, when it is communicated, the force is not instantly annihilated on removing the cause, then such a property of matter may be substituted in the preceding reasoning instead of magnetism, the same deductions may be made, and similar consequences may be expected to result. On reviewing the known properties of matter, electricity seemed to be the only one which satisfied the requisite conditions: imperfect conductors placed in the neighbourhood of excited bodies do not immediately attain a state of electric equilibrium; and when they have arrived at that state which is due to such a position, on the removal of the excited body they do not instantly return to their former state. I therefore made the following experiments.

Experiment 1.

A needle was made of thin sheet brass, of the form, fig. 5, having two circular ends 1.25 inches in diameter, and the distance of their centres 3.1 inch; it had its edges coated with sealing-wax. Another similar needle was made, about one-third larger. One of these needles was suspended from a filament of silk, of several feet in length, at the distance of .25 inch above a circular glass plate six inches in diameter, which was fixed by sealing-wax on a disc of box wood 2.5 inches in diameter: this latter was screwed upon a wooden axis. No electricity was communicated either to the needle or the glass disc, except that which resulted from cementing the latter by sealing-wax to the box wood on which it was fastened, and perhaps the coating of wax on the edges of the needle might have given it a very slight charge. The glass disc was now made to revolve at about 38 revolutions in a minute: there was a small motion of the needle in the same direction.

Experiment 2.

The same apparatus being employed, a large stick of common sealing-wax was excited and applied two or three times to the needle, in order to charge it. As soon as it was quite at rest, the glass disc was made to revolve more slowly than before, when the needle began to deviate in the direction of rotation, and followed the glass. Some doubt being expressed by a friend who was present as to the influence of the air set in motion by the apparatus, the electric charge of the needle was increased, and the velocity of rotation was diminished: the needle deviated as before, but much more rapidly. To remove some doubt as to the influence of any twist in the silk; whilst the needle was revolving in one direction, the rotation of the glass disc was reversed; after a short time the motion of the needle was destroyed, and it returned in the contrary direction.

For the purpose of ascertaining still more satisfactorily the effect which might be ascribed to the influence of air, the following experiment was made.

Experiment 3.

The apparatus was left many hours, in order that the electricity of its parts might, as far as possible, be dissipated. On returning to it, the glass disc was made to revolve with very great rapidity: not the slightest sensible effect was produced on the needle.

Experiment 4.

The needle was now electrified, and the same velocity being given to the glass plate, and all the other circumstances being the same,—the needle followed slowly. On trying various degrees of velocity, it was observed, that the effect on the needle was greatest when the motion of the glass plate was slow. A velocity of about five turns in a minute produced a considerable effect.

Experiment 5.

A stick of excited sealing-wax was substituted for the brass needle, and placed at the distance of about half an inch above the glass disc. On giving a slow motion to the glass plate, the wax needle followed in the same direction, and continued to move through many revolutions, notwithstanding the torsion of the silk; but on giving the glass a very rapid rotation, the sealing-wax needle remained nearly immoveable.

Experiment 6.

A new apparatus was constructed, which is represented in fig. 1. It consisted of a circular piece of wood A B, 9 inches diameter, having a hole of 1.5 inches in the centre, supported by three legs, two of which only, C and D, are represented in the figure; these, by screwng into it, served to adjust its height. A cylinder of thick millboard F G, 5 inches high, was glued together, and was fastened to the board A B. In two very small recesses in its upper part rested a narrow bridge of glass or of wood *b b*, for the purpose of supporting a needle. A circular ring of pasteboard *c c*, was fixed near the top, which supported an engraved circle divided into degrees. A square piece of millboard P P, having a circular opening rather less than the diameter of the cylinder, rested on the top of it. Upon this was placed another millboard cylinder Q Q, of rather larger diameter, and 2¼ inches high. Another cylinder was fitted to the outside of this, in order to admit of fixing screens of muslin and various substances to the under side of the upper cylinder.

Through the aperture in the centre of the wooden plate A B, a steel axis working in a brass frame passed up into the cylinder. The frame was supported by being screwed to a block of wood at S. On the lower end of this axis a pulley H, was fixed, to which motion was given by connecting it with a common jack. On the upper end of this axis a thin brass chuck K 2 inches in diameter, was fixed, to which was attached a wooden disc Z, 5 inches in diameter. Three wooden screws *a a a*, rose from this plate, and supported a disc M, of any substance on which experiments were to be made.

A needle made of pasteboard covered with sealing-wax, and having an agate cap in its centre, was supported on the point of a steel sewing needle fixed in a wooden bridge .05 inch square. A plate of glass revolved below. The following were the times in which different revolutions were performed.

When the glass disc made 26½ turns per minute.			When the glass disc made 19¼ turns per minute.		
Number of Revolutions.	Time.	Difference.	Number of Revolutions.	Time.	Difference.
	′ ″	″		′ ″	″
0	21 0	49	0	34 0	52
·25	21 49	26	·25	34 52	28
·5	22 15	15	·50	35 20	17
·75	22 30	15	·75	35-37	15
1	22 45	42	1	35 52	45
2	23 27	35	2	36 50	43
3	24 2	35	3	37 35	40
4	24 37	33	4	38 18	39
5	25 10	32	5	38 58	38
6	25 42	32	6	39 37	40
7	26 12	29	7	40 15	40
8	26 44	38	8	40 55	43
9	27 16	30	9	41 35	41
10	27 45		10	42 18	

In the latter of these experiments, at the seventh revolution, the passing of a carriage may have slightly affected the remaining revolutions.

Experiment 7.

A thin disc of copper, such as is used for engraving upon, about seven inches in diameter, was connected by wax with the glass plate employed in experiment 1, and the same apparatus was used with a needle of sealing-wax suspended above: on making the copper plate revolve rather quickly, the needle followed it, and went through two revolutions. Several plates of lead, pewter, brass and copper were tried, and the results were similar.

For the purpose of procuring more powerful and durable charges of electricity, I placed a small excited electrophorus on the three screws *a, a, a,* fig. 1, the copper plate was placed upon it. A very thin slip of glass *b, b,* formed a bridge, on the centre of which various needles were supported. The effects which resulted were not constantly the same, aad the unequal distribution of electricity on the electrophorus, combined with the action of the glass bridge, seemed to present very complicated results. I shall only detail one of these experiments.

Experiment 8.

The apparatus just referred to being employed, and the electrophorus without a copper plate being used, a needle of thin brass, similar in shape to that of experiment 1, with an agate cap, was supported by a fine needle point fixed by cement to the centre of the glass bridge. The brass needle rested nearly at right angles to the glass bridge (strictly at 98° from it).

On giving a slow motion to the electrophorus, the brass needle immediately advanced in the same direction about 10°, then stopped, then returned back to its original position, and, after a few vibrations, settled at a point 2° in advance of its original position. On increasing the speed of the electrophorus, the needle again advanced, and settled at between $3^{\circ}\frac{1}{2}$ and 4° in advance of its first position. The velocity was again considerably increased, when the brass needle began to oscillate in a vertical direction, and the stationary point advanced to 9° or 10°, where it remained steadily, except that the vertical motion continued

The velocity was again increased, when the stationary point moved on to 12° from its position at the beginning of the experiment. On stopping the motion, the needle advanced 115°, and then rested.

These experiments appear to me to admit of explanation, on precisely the same principles which I had previously applied to those relating to magnetism. It seems that electricity excited by induction is not *instantly* destroyed by removing the inducing body; nor is it instantly excited, to the full extent which the circumstances admit, on presentig an excited to an unexcited body. Admitting this principle, the whole of the reasoning which was given at the commencement of this paper, relative to rotations from magnetic causes, applies equally to those which arise from electricity.

The velocity with which electricity travels, has been urged as an objection to this explanation of the phenomena; and the immense rapidity with which the metals conduct that fluid, renders it difficult to admit that the motions employed in these experiments are at all comparable with that of electricity. There are two circumstances that may be stated as some explanation of this difficulty: the first is, that although the forces thus set in action are exceedingly weak, yet, from the nature of the apparatus, they act constantly for a considerable time, and in the same direction; thus gaining, by the duration of their total action, a power far superior to the almost infinitely small one which arises from the transient action of each part: the second is, that although the velocity with which electricity of considerable intensity moves through good conductors is excessively great, I am not aware of any experiments which prove that the small inequalities which are produced in a very weakly electrified body by induction from some neighbouring body, are removed with the same velocity. The analogy of a fluid similarly circumstanced, would lead us to infer, that when the deviations above the level are minute, the return to it will be more slow.

Of Screens.

Although the preceding experiments, when repeated under various circumstances, had convinced me that the rotations which had been observed were not attributable to currents of air set in motion by the apparatus employed, I was desirous of examining what effect would result from the interposition of a screen. I therefore suspended the needles I employed by a silver wire, $\frac{1}{500}$ inch in diameter, and about two feet in length, which was contained in a glass tube, supported over the middle of the apparatus represented in fig. 1. This tube and wire passed through a circular hole in the centre of the glass plate R. The bridge *b b*, was removed, and a screen of fine muslin was stretched across the bottom of the lower cylinder Q, by means of the external one *c c*. This screen and the needles were occasionally changed.

Experiment 9.

A brass needle, having a very fine thread of spun glass cemented to one end by a little wax, was suspended by the silver, about $\frac{1}{8}$ inch above a screen of coarse gauze (not silk). A pewter disc, of 4 inches in diameter, was placed on the revolving stand, $\frac{3}{16}$ inch below the gauze screen. Neither the needle nor the disc were electrified; and they were completely protected from any currents of air moving about in the room in which these experiments were made.

The three first oscillations of the needle were:

1st. 127.8
2nd. 126
3rd. 127.8

The disc was then set in motion, and kept revolving at about 78 turns in a minute. After six minutes no result was apparent in the needle different from those which had been observed before the motion commenced. This seems to show that the action of air, if it is driven through the screen, is insensible.

Experiment 10.

Having placed a screen of coarse gauze above the pewter plate at $\frac{1}{8}$ inch distant, an excited needle of sealing-wax was substituted for that of brass. I observed the extent of the oscillations, and then setting the plate of pewter in motion, I was surprised to find that the returning arc (contrary to the direction of the plate's motion) was almost invariably increased, generally at the first, and almost constantly at the second oscillation, and that the arc in the same direction was diminished.

So unexpected a result naturally induced me at first to attribute it to some error in the observations; but on repeating it many times, and having measured the greatest error which the means I employed were susceptible of when used even a little carelessly, I found that half a degree was the greatest error I could make in reading off the oscillations, and that this retrograde motion was in many cases equal to three, four, or five degrees; so that the measure of the observed fact was from four to eight times the extent of the greatest possible error of observation, even admitting it to take place at both extremities. It is true, I did not always observe this retrograde motion; but in twenty experiments the needle, screen and plate, not being always at the same relative distances, I noticed it seventeen times. The next step seemed to be to find some mode of encreasing the unequal distribution of electricity on the metallic plate, supposing that to be the cause of the phenomena. I therefore made a very small lamp, which being lighted, was placed on the stage L of the revolving apparatus, immediately under the copper. It was afterwards found convenient to make a small aperture in the lower millboard cylinder, just opposite the platform supporting the lamp, for the purpose of lighting it when requisite: this opening was closed by a piece of mica, fastened on a card.

Experiment 11.

A small stick of sealing-wax being suspended from the silver wire above a leno muslin screen, the pewter plate was supported just above the lamp, and under the screen, the following observations were made:

Oscillations of the needle.	Diff.	½ Diff.	Mean point.
°	°	°	°
R.... 16			
L.... 18	2	1	17
The lamp was now lighted.			
R.... 15.5	19.5	4.75	20.25
L.... 25			
R.... 18	13	6.5	24.5
L.... 31			
R.... 14	20	10	24
L.... 34			
R.... 14.25	19	9.5	23.7
L.... 33.2			
R.... 16	12	6	22
L.... 28			
R.... 21	4	2	23
L.... 25			
R.... 20	9	4.5	24.5
L.... 29			

The wax now became more heated, and gradually bent until the glass index touched the graduations. The number of revolutions of the pewter plate and lamp were 56 per minute.

Experiment 12.

The same apparatus being used, a brass needle was substituted for that of sealing-wax; it was suspended ½ inch above the leno muslin screen, which was ⅛ inch above the pewter plate; these distances were the same in Experiment 11. The plate and lamp made 56 revolutions per minute.

The observations were made as in the last experiment. The apparatus being at rest, the mean point was 2°.

Oscillations.	Diff.	½ Diff.	Mean point.
0	0	0	0
R......0	4	2	2
L......4			
Lighted lamp, and set it and plate revolving.			
R —.5	11	5.5	5
L + 10.5			
R — 1.5	13.5	6.75	5.25
L + 12			
R — 8	18.5	9.25	1.25
L + 10.5			
R — 5.8	9.8	4.9	—0.9
L + 4			
R — 3	9.5	4.75	+ 1.75
L + 6.5			
R — 9.5	19.0	9.5	0.
L + 9.5			
R — 13.5	25.5	12.75	—0.75
L + 12			
R — 15	32	16.0	+ 1.0
L + 17			
R — 14	26	13.0	— 1.0
L + 12			
R — 11	20	10	— 1.0
L + 9			

The lamp burned with a very dull flame, $\frac{3}{16}$ inch long, the top of the flame about $\frac{5}{8}$ inch below the pewter disc; it was lighted by a small stick, which burnt with a flame, and gave more heat than the lamp during the short time it was under the plate. This experiment lasted about twenty minutes.

Experiment 12.

The same brass needle being used in the same apparatus; the distances remaining as before, the lamp was trimmed, so as to give a much brighter flame.

Oscillations of needle.	Diff.	½ Diff.	Mean point.
°	°	°	°
L. . . . . 0 R. 2	2	1	+ 1
The lamp was now lighted, and set in motion.			
L — 5 R + 2.5	7.5	3.75	—1.25
L — 1 R + 6.5	7.5	3.75	+ 2.75
L — 3.5 R + 6.5	10.0	5.0	+ 1.5
L — 5.0 R + 7.0	12.0	6.0	+ 1
L — 4.5 R + 6.0	10.5	5.25	+ 0.75
L — 3.5 R + 9.5	13.0	6.5	+ 3.0
L — 5.4 R + 4.5	9.9	4.95	— .45
L — 4.5 R + 4.0	8.5	4.25	— .25
L — 2.0 R + 5.5	7.5	3.75	+ 1.75
L — 3.0 R + 7.0	10.0	5.0	+ 2.0
L — 4.0 R + 6.0	10.0	5.0	+ 1
L — 4.0 R + 6.0	10.0	5.0	+ 1
L — 3.5 R + 9.0	12.5	6.25	+ 2.75
L — 10 R + 11.5	21.5	10.75	+ .75
L — 6.5 R + 10.0	16.5	8.25	+ 1.75
T — 6.0 R + 5.0	11.0	5.5	—0.5
L — 8.0 R + 15.0	23.0	11.5	+ 3.5
L — 14.5 R + 16.0	30.5	15.25	+ 0.75
L — 15.0 R + 17.5	32.5	16.25	+ 1.25
L — 19.5 R + 17.0	36.5	18.25	—1.25

Soon after the commencement of the experiment a mist arose within the case, which continued about five or six minutes: this frequently happened in the other experiments. The duration of the observations was about twenty-five minutes in the two preceding experiments.

On examining the column of the mean point of the needle, I am not inclined to draw any inference respecting it: whatever action may have been exerted on it, does not appear to have produced a marked or decisive effect on that point. I was induced to insert them, from the circumstance of the remarkable and gradual increase of the arc of vibration in both instances. This will be observed on looking down the columns of half differences, which is, in fact, the amplitude of each oscillation about the mean point.

The observed retrograde movement in the eleventh experiment being so small, and yet exceeding many times the greatest possible error of observation, I was now desirous of enquiring whether, amongst the accidental circumstances to which the needle was exposed, there might not be found some one to whose influence this apparent anomaly might be ascribed. The number of causes to which it might be attributed was considerable; but I hoped by variations in the circumstances of the experiment gradually to eliminate them.

The following list comprised the greater part of them. The reverse motion might arise from, 1. Currents of air in the room; 2. From currents of air excited within the apparatus; 3. By air driven through the gauze screen; 4. By heated air rising from the metal plate heated by the lamp; 5. By the vibration excited in the apparatus by the motion of the jack; 6. By the torsion of the silver wire to which the needles were attached; 7. By the electricity of the bridge; 8. By the bending of the wax needle.

The first of these causes, namely, the action of currents of air in the room in which these experiments were made, was obviated by the form of the apparatus in which the needle was afterwards inclosed, and care was taken to render all the parts of it as close as possible.

The second source of this retrograde motion might be supposed to arise from the movement impressed on the air within the lower cylinder of millboard being communicated through the screen, not by actually passing through its apertures, but by the vibrations it might impress on it. The fact of the direction of the motion in the lower cylinder being in the contrary direction to that of the needle in the upper one, was exceedingly unfavourable to this hypothesis.

Before I relate the various experiments which I made, with the view of successively placing each of these causes out of action, I think it may be necessary to detail a few additional experiments, in confirmation of the fact.

Experiment 13.

Using the same apparatus, I substituted a disc of copper, 5 inches in diameter, and $\frac{1}{16}$ inch thick, instead of that of pewter; the sealing-wax needle was suspended half an inch above the leno screen; the lamp was placed at one inch from the edge of the copper plate, towards its centre:—the flame was large and smoky.

Oscillations of needle.	Diff.	½ Diff.	Mean point.
°	°	°	
L ... 347.8 R ... 343.6	4.2	2.1	345.7
L ... 347.0 R ... 343.5	3.5	1.75	345.25
The lamp was now lighted.			
L ... 348 R ... 344	4	2	346
L ... 339.5 R ... 346	6.5	3.25	342.75
L ... 331.2 R ... 337	5.8	2.9	334.1
L ... 330 R ... 339	9	4.5	334.5
L ... 334 R ... 337	3	1.5	335.5
L ... 330 R ... 342.5	12.5	6.25	336.25
L ... 332.5 R ... 356.5	24.0	12.0	344.5
L ... 341.4 R ... 396	54.6	27.3	368.7

At one part of this experiment the needle became stationary, as if it had arrived at the end of its arc; it then went on again in the same direction as before. This stationary point should perhaps be considered as the extreme right of one vibration, and the extreme left of the next: I have noticed it more than once in these inquiries. The column of the mean point of the needle has, in this experiment, marked the same retrograde motion on the first application of heat, and the same motion in advance, when that heat has continued, as in former ones.

Experiment 14.

The same apparatus; the copper disc revolving at ⅜ inch below leno screen; the lamp in the centre $\frac{1}{2}$ inch below the copper; the sealing-wax needle $\frac{7}{8}$ inch above screen. I shall, in this instance, only give the gradual progress of the mean position of the needle, as calculated from the observations.

Position of the mean point.

°
203
203.5

Lighted the lamp.

200
190.1
189.9
199.9
205.3
213.3
212.0
211.5
217.3
216.5
221.3
224.6

This experiment presents the same results as the preceding one; and shows, that the observed fact is not influenced by the eccentric position of the source of heat.

As the silver wire, from which the needles were suspended, had been unwound from a small cylinder, I thought it probable that it might be a little twisted; in which case it would, from the weight of the needle, untwist until counterbalanced by the elasticity of the wire. If, in these circumstances, heat were applied, it might happen that the elasticity of the wire would be altered, and consequently, from that circumstance alone, that the needle might change its position. If this were the case, on reversing the direction of rotation of the plate, the torsion, which was previously supposed favourable for the reversed motion of the needle, would now act against it; I therefore made the following experiment.

Experiment 15.

The motion of the copper plate was reversed, and reading in the new direction, the following were the mean positions of the wax needle.

°
15.6
16.0
The lamp lighted.
15.0
Observation lost.
2.0
1.1
31.5
37.2
58.3
45.5
50.3

The greatest regression of the mean point of the needle was nearly 14°; so that if torsion had any influence on the former experiments, it was unfavourable to the production of the retrograde motion.

Experiment 16.

A screen of sheet zinc, about $\frac{1}{16}$ inch thick, was placed under a needle of sealing-wax, at about $\frac{7}{8}$ inch below it. The lamp was placed on the platform below, having its wick at the distance of about an inch from the zinc. The following observations were made.

Mean position of the needle.
249°.0
249.25
Lamp lighted.
250.75
256.25
258.75
256.25
254.75
260.25
258.75
262.25
260.5
261.25
261.5
268.5

In this case, where a source of heat was revolving under a metal plate, the needle suspended above did not retrograde, but moved on in the same direction as the heat: the small comparative recession, at the sixth and seventh observation, not being of sufficient magnitude to be noticed, and it never having returned as far back as the point from which it started.

From this experiment it appears, that, whatever may be the cause of these movements, it does not arise from any vortex produced by the moving part of the apparatus, nor is it owing to the passage of any air through the screens.

In order to prove that no tremors arising from the moving power, and communicated through the solid parts of the apparatus, had produced these effects, the following experiment was made.

Experiment 17.

The apparatus being in precisely the same state as in the last experiment, the lamp was not lighted, and the following observations were registered.

Oscillation of wax needle.	Diff.	½ Diff.	Mean position.
°	°	°	
L ... 259	4	2	257
R ... 255			
L ... 258.5	2.5	1.25	257.25
R ... 256			
The apparatus was set in motion; the lamp revolving, but not being lighted.			
L ... 258.8	2.8	1.4	257.4
R ... 256			
L ... 258.8	2.4	1.2	257.6
R ... 256.4			
L ... 258.6	2.1	1.05	257.55
R ... 256.5			
L ... 258.8	1.6	0.8	258
R ... 257.2			
L ... 258.2	1.4	0.7	257.5
R ... 256.8			
L ... 258.2			

This experiment clearly proves, both from the stationary position of the mean point, and from the decrease of the arc through which the needle oscillated, that no vibrations are communicated from the jack to the needle.

I now tried the effect of winding up the jack, and found that it did not increase the arc half a degree on each side, although it caused the needle and silver wire to oscillate slightly, as a pendulum.

Considering it possible, by means of an agate cap, to suspend the various needles employed in these experiments, with sufficient delicacy to allow them to obey the minute forces under investigation, I made the following experiment.

Experiment 18.

A card of the same form and size as the brass needle described in Experiment 1, covered with sealing-wax, having an agate cap in its centre, was balanced on the point of a sewing needle, cemented to a glass bridge, which rested on the screen. The wax card was about 1.5 inches above the leno screen; under which, at the distance of .3 inch below, the copper plate was placed. The lamp was placed below the copper, at the distance of .75 inch from its edge. No motion was communicated to any part of the apparatus; the waxed card needle rested at 168°. The lamp was lighted, and at the end of 75″ the needle had moved 8°; it gradually advanced towards that part of the copper immediately above the lamp, and at the end of four minutes and a half became stationary at 87°.5, a point not precisely over the lamp, but a little on one side of it.

Experiment 19.

The same apparatus and relative distances being used, no motion was employed; a piece of red-hot iron, one inch long, by half an inch broad, was placed on the platform below the copper, instead of the lamp. The red-hot iron was placed under 210°, about one inch below the copper; and the waxed card needle rested at 143°. The experiment began at

4^{h} 23^{m}
4 28 no effect.
4 32

The nearest end of the needle had moved over 47°, to a spot not quite above the edge of the hot iron, and about 10° from its centre.

In order to get rid of the electrical effect of the glass or wooden supports, which I have called bridges, and which sometimes supported the needles, I placed a cork on the centre of the screen, immediately above the centre of the copper disc; into this was fixed the point of a sewing needle, on which the agate caps of the needles rested. I conceived this arrangement necessary, because, when no screens were employed, and the distance between the needle and the revolving plate was considerable, it sometimes happened that the needle followed the copper in its motion until it came over the supporting bridge, where it sometimes rested, instead of continuing its revolutions as long as the copper plate was kept in motion.

Experiment 20.

Using a wax card needle supported by a cork placed over the centre of the leno screen, and the lamp placed in the centre, under the copper plate, a slow motion was given to the lamp and plate. After some minutes, the needle had receded in a direction contrary to that of the motion of the plate through 35°. This experiment was repeated several times, and I observed that the needle, (if it moved at all,) generally went in an opposite direction to the plate; and that its motion was often so slow as to be imperceptible to the eye, although it could be very distinctly perceived after the lapse of a short time, by its altered position. In various repetitions of this experiment, the needle slowly retrograded from 10 to 90 degrees.*

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* In one experiment which lasted above half an hour, and in which I had the advantage of Mr. Christie's assistance, the needle slowly retrograded over 157°. In this case the motion was rather slower than that of the minute hand of a watch.

For the purpose of determining whether the air, heated by the upper surface of the metal disc, acted on the needles, as it does on the vane of a smoke-jack, I made the following experiment.

Experiment 21.

Having formed a needle of sheet brass, thinner and lighter than those hitherto employed, but of the same form, I bent the circular parts, at the extremities, in such a manner, that they formed angles with the longitudinal axis of the needle; one of about 5°, the other about 15°. This being balanced above the copper plate, the lamp was lighted below it, but no rotation was communicated to the apparatus. The needle soon began to acquire vibrations in a vertical plane; but after five minutes' exposure there was no perceptible rotation. I now bent the circular extremities of the needle so as to form angles of about 35° and 40°. On replacing it, and lighting the lamp, the needle moved about 23°, in a contrary direction to that which the impinging air ought to have produced; it then returned, and went on through several revolutions in the opposite direction. The needle employed in this experiment projected a little beyond the copper plate, and would therefore receive some of the heated air passing from the lamp along the under side of the plate, to which cause the vertical vibrations may perhaps be ascribed.

From this experiment it appears, that if the needles are made flat with ordinary care, rotation will not ensue from the action of the heated air.

I shall now briefly restate the various causes to which these rotations might be ascribed, and refer back to the experiments which refute each supposition.

1st. The various currents of air in the room could not produce an almost uniform result. Considerable care was taken not to move about more than was requisite, and the experiments were generally made after the apparatus had been left, and the room shut up for some hours previous. In all but the earliest experiments the cylindrical millboard cases, in which the needles were placed, effectually excluded all currents of air exterior to them.

2nd. The rotation of a current of air produced within the lower cylinder by the motion of the lamp, metal plate, and the wooden platform supporting them, would, if it produced any effect, tend to make the needle rotate in the same direction. But we have seen from many experiments, that when a muslin screen is interposed, the rotation at the commencement is in the opposite direction. Also in Experiments 18 and 19 there was no rotation given to the apparatus, and yet the needle advanced; and in Experiment 16 a sheet of zinc prevented the air set in motion in the lower cylinder from communicating with that in the upper.

3rd. That air driven through the screen was not the cause of these motions, appears from their taking place when the screen was impenetrable to it. In Experiment 9, where no electricity was employed, a rapid rotation was kept up during six minutes immediately under a gauze screen, and no motion was produced in the needle above it.

4th. Perhaps the most probable of these suppositions is, that the motions result from the effect of currents of air which rise from the surface of the heated metal disc: these will cause other lateral currents, arising from the cold air flowing in to supply the place of that which has been heated, and has ascended. Such an hypothesis might explain Experiments 18 and 19, if, on measuring its influence, the cause were found sufficiently powerful; but it appears from Experiment 21, that when the needles are not visibly bent in the form of vanes, this cause is not sufficient to produce the effect, although it acts in the same direction. The heated air above a revolving plate will revolve in the same direction; and if it pass through the interstices of the muslin, it ought to impress motion in the same direction as the plate from which it rises. The heated air above the zinc screen had not this rotatory motion; and we have seen by experiment 21, that its vertical action was not sufficient to produce the effect.

In many of the experiments no heat was employed; and yet the same motions resulted, as is apparent by Experiments 5 and 6 for direct motions, and by Experiment 10 for the retrograde.

5th. Vibrations excited in the apparatus by the motion of the jack. This cause was decisively refuted by Experiment 17, in which they were measured, and found to be quite insensible. Also in Experiments 18 and 19 the apparatus was not in motion, yet the needles did move.

6th. The torsion of the silver wire to which the needles were attached. Until I had measured this torsion by reversing the direction of the moving plate, as in Experiment 15, I had some suspicion that the heat of the lamp might have altered the elasticity of the wire; and although I did not then see how the reverse motion could be well accounted for, yet I was desirous, if possible, of removing altogether this disturbing action. Some of the previous experiments in which no heat was employed, and many of the subsequent ones in which no wire was used, effectually refute this supposition.

7th. The electricity of the bridge might be supposed to produce some of the motions.

It was more necessary to get rid of the action of this cause than of any of the others, because electricity, modified indeed by the circumstances under which it was placed, did, in my opinion, produce the whole of these curious phenomena. The action of the glass or wooden support was apparent, and is noticed in the remarks on the 19th Experiment; but as it was not employed in any of those experiments in which the balance of torsion was used, and as it was dismissed from several of the latter ones in which reversed motions of the needle appeared, they cannot be attributed to this cause.

8th. The bending of the wax needle.

The application of heat after some time altered the form of the wax needle; it sometimes became so soft as to bend by its own weight down to the graduated circle. This change, however, only took place at the latter part of the experiment in which it occurred, and the phenomena recorded usually appeared at a much earlier period, when if it had commenced the change was quite imperceptible. In those experiments in which heat was not employed this cause was absent; and in many of those in which it was applied, a needle of card, or of thin brass covered with sealing-wax, or a mixture of resin and shell lac, was used, in which no bending took place.

Having shown that none of the causes to which I have alluded are common to all the experiments in which the phenomena occurred, I shall now offer that explanation of them which appears to me to be warranted by the observations that have been recorded in the preceding part of this paper. They naturally divide themselves into two classes. 1st. Those in which the motion communicated is in the same direction as that of the revolving plate. 2ndly. Those in which the motion of the needle is in a contrary direction to that of the plate.

The instances in which the needles employed followed the directions of the revolving plate are so numerous, and the extent of their motion so great, that no doubt can remain of the fact, and some of them may be repeated with a very simple apparatus. The circumstance of one of the bodies employed being generally an electric, such as wax, resin, glass, and of the increase of the observed motions when these were previously excited by friction, or by a change of temperature during the experiment, naturally points out electricity as the cause. It is indeed the only one common to all the cases detailed.

If it be admitted that induced electricity is neither acquired nor given up in an instant of time, then it necessarily follows, from the reasoning I have explained at the commencement of this paper, that such motions must arise.

2nd. The case of a retrograde motion is much more difficult of explanation. Its existence rests on measures of much smaller magnitude, and it is by no means so easily re-produced, requiring some delicacy both in the adjustment of the apparatus, and in the observations of the needle. I have myself observed it so frequently, as to have not the smallest doubt of the fact. I have occasionally shown it to several friends; although I have not in every instance succeeded in this.

To have omitted to state in this paper the fact of such a retrograde motion would have been uncandid, because it is one which is strongly opposed to all the reasoning that it contains. To abstain from giving any explanation, however imperfect, that might reconcile it with my view of the subject, would be to leave untouched a very powerful, and almost solitary argument against the explanation which I have proposed both of magnetic and electric rotation. I shall therefore attempt to show, not merely that it is not repugnant to the principle which I have already explained, but that that very principle may, in certain circumstances, produce the retrograde motion, and that in others nearly similar, it shall not take place; thus not merely showing the possibility of the fact, but accounting for its apparently capricious nature.

In figure 2, N represents the end of an excited needle, situated above a metal plate C, and having a muslin screen G interposed between them. All three being at rest, the screen and the metal plate will become electric by induction, and one of the two following arrangements must take place: either the electricity developed on the screen and on the plate will be of the same species, in which case it will be contrary to that of the needle N; or the electricity on the screen and the metal plate will be of different kinds, in which case one of them must be of the same species, the other of a different one from that of the needle.

In the first case let B B represent the curve of the intensity of induced electricity on the muslin screen, that is, let each of its ordinates be supposed proportional to the electricity at the corresponding point on the screen; then, part of the influence of the electrified body N passing through the interstices of the screen, produces on the metal plate the same kind of electricity as it did on the screen. Let the curve of electric intensity in the metal plate be A. Whilst the metal plate C remains at rest, both its electricity and that of the screen tend to draw the needle N directly towards it; but if the plate C move in the direction of the arrow with an uniform velocity, and if induced electricity be not instantly annihilated, then there will arise a new electric equilibrium, and the curve representing its intensity and position will have advanced towards that part to which the motion is directed, and may be represented by the dotted line *a*. The influence of this electricity, which is of the same kind with that on the screen, and which consequently it repels, will be, that the electric curve on G must be driven back in the opposite direction: let it be represented by *b*, *b*. Now the attraction of *b*, *b* on the needle N tends to give it a motion in the direction contrary to that which is impressed on it by the attraction of *a*, which is on the opposite side of the perpendicular, dropped from N to the metal plate. That proportion of the resolved action of *b*, *b*, which communicates horizontal movement, is larger than it is at the greater distance of the plate C, and the whole force acts at a shorter distance: it may therefore exert a more powerful influence in turning the needle in that direction, than a stronger force at a greater distance, C acting more unfavourably. Thus the needle may, by the advance of the plate C, be caused to retrograde. It does not, however, follow, that this must always happen, for the susceptibility for induced magnetism in the plate may be much greater, and it may retain it much longer than the screen, and in this case we ought to anticipate a motion of the needle in the same direction as the plate.

Let us now examine the case of one of the bodies possessing the same kind of electricity as the needle. Let the same letters in figure 2 represent the electricities when the plate is at rest; thus, when it moves in the direction of the arrow, the electric curve will assume the position of the dotted line *a*; and since it is of an opposite kind to that of the screen, it will attract it, and will tend to draw the electric curve B on the screen into the position *b*, *b*, marked by the dotted lines. In this case the electric forces are both on the same side of the perpendicular from the needle; but since they are of opposite kinds, one will attract, the other will repel the needle; and according to the strength of these forces, it will advance, be stationary, or recede. This explanation accords with the apparently capricious nature of the fact; the circumstances on which a direct or a retrograde motion depends are so numerous, and in the present state of our knowledge we have so few data for calculating their influence, that it is not surprising that the result of any given combination should be uncertain. Besides the relative distances, we ought to be acquainted with the relative conducting powers of the screen and plate,—the intensity which can be excited in each by a given inducing source—the quantity of that inducing action which can be transmitted through the interstices of the screen—the time each body takes to acquire and give up electricity—before we can attempt accurately to predict the result of any proposed arrangement.

There is another circumstance in which the above explanation agrees well with the observed facts; since the motion communicated to the needle is the difference of two forces, each of which is very small, it must itself be smaller than either; and in case of any approach to equality in those opposing forces, it will become exceedingly minute.

With respect to the action of heat in encreasing these rotations, I am inclined to conjecture, that the distribution of electricity on the metallic plate is altered by the application of heat; that the part under the needle becoming electrified, produces in it induced electricity; and thus the plate being made to revolve, the rotation of the needle follows from the explanation previously given. It may, however, happen, that the wax needle becomes itself electric by the heat it receives from the plate below it.

It will naturally be enquired, whether a parallel to this retrograde motion cannot be produced with magnets. I have made a few trials with a plate of soft iron, an iron gauze screen, and neutralized magnets, but have not succeeded. I found the needles stationary over whatever point they were placed.

I shall conclude this paper with mentioning some of the methods by which I observed the angles passed over by the needles; this is the more necessary, because it was desirable to remove all the parts of the apparatus as far from them as could be conveniently done.

If the angle was small, a fine thread of glass was made to project from the end of the needle, and a lamp was placed in such a position as to throw its shadow on the graduations below; this of course would only do for the measure of a few degrees, and was inconvenient by day.

In some instances I used a plano-convex lens cut into halves, one half being fixed as much before the other as the distance of the needle from the graduations required; this brought the image of the glass thread and the graduations into the same plane; but as the lens I had cut was of too short focal length, this plan was not often adopted.

I occasionally employed a narrow ring of glass, on one half of which the graduated card circle was pasted; the back of the glass ring was blackened. By placing the eye above the needle in such a position that the direct image of the glass thread covered that which was reflected from the black glass, all parallax was avoided; and I found that, without great carelessness, repeated observations never differed more than a quarter of a degree.

Another method which I made use of was, to draw a fine line along the needle; and having made a small hole in the centre of the glass plate which covered the apparatus, I fixed an index of card, which could revolve about it as a centre; this index extended to a graduated circle fixed on the same glass cover. When I wished to observe the position of the needle, I brought one edge of the index to coincide with the line on the needle. There were some advantages in this method, but it had the inconvenience of obliging me to touch the apparatus at each observation.

XXXI. *Case of a lady born blind, who received sight at an advanced age by the formation of an artificial pupil. By* JAMES WARDROP, *Esq. F. R. S. Edin. Surgeon Extraordinary to the King, &c. Communicated by the President.*

Read June 15, 1826.

As imperfections in the original structure of our organs of sense, which are remediable by art, are extremely rare, and as cases of successful operations on these organs essentially contribute to illustrate their functions, as well as to throw light on the operations and developement of the human mind, the following instance of vision being imparted to a lady born blind, by an operation at an advanced period of life, will, it is hoped, not be considered unworthy of being submitted to the consideration of the Royal Society.

The case, besides establishing the curious physiological fact, that the nerve of the eye can remain fit to receive the impressions of external objects, though totally excluded for a long series of years from the performance of that function, claims a much higher interest in a philosophical point of view; some of the facts here detailed confirming in a remarkable manner what BERKELEY had predicated of "a man born blind being made to see," in the 79th Section of his "New Theory of Vision," published in the year 1709. He says, "a man born blind being made to see, would, at the first opening of his eyes, make very different judgements of the magnitude of objects intromitted by them from what others do. He would not consider the ideas of sight with reference to, or as having any connection with the *idea* of touch." It may also be observed, that in the present case the blindness was more complete, and the period at which vision was acquired was much later in life, than in any instance which has hitherto been recorded.

The lady, whose case forms the subject of this paper, was observed, during the first months of her infancy, to have something peculiar in the appearance of her eyes, and an unusual groping manner, which made her parents suspect that she had defective vision. When about six months old, she was placed under the care of a Parisian oculist, who performed an operation on both her eyes, with a view to afford her sight. The operation on the right eye was, however, followed by violent inflammation, and a collapse of the eye-ball, thus causing a complete destruction of the organ of vision. The operation on the left eye, though equally unsuccessful in attaining its object, was not followed by any alteration in the form or size of the globe. From the account stated by her friends, it was impossible to form any correct notion of the state of her eyes previous to the operations which were performed. It seems, however, extremely probable that the blindness, which was attempted to be remedied, had been produced by congenital cataracts, and that these operations had for their object the removal of the opaque lenses.

From the above early period she had continued totally blind, being able merely to distinguish a very light from a very dark room, but without having the power to perceive even the situation of the window through which the light entered; though in sunshine or in bright moonlight, she knew the direction from whence the light emanated. With regard therefore to the degree of sight, this lady was more completely blind than the boy in the celebrated case related by Mr. CHESELDEN, in the 35th volume of the Transactions of the Royal Society; for in that instance the boy knew black, white, and scarlet apart from one another; and when in a good light he had that degree of sight, which generally continues in an eye affected with cataract; whereas in this lady, the pupil being completely shut up, no light could reach the retina, except such rays as could pass through the substance of the iris.

When she was placed under my care she had reached her 46th year. The right eye-ball was collapsed, but the left retained its natural globular form. The cornea of this eye was transparent, except at one point near its circumference, where there was a linear opacity, which had probably been the cicatrix of the wound made during the operation in her infancy. The anterior chamber of the eye was of its natural capacity, but I could not distinguish any vestige of a pupil, some streaks of yellow lymph being deposited in an irregular manner over the central part of the iris. There was every reason to believe that the retina was sound; for though she could not perceive objects, nor had any notion of colours, yet the circumstance already mentioned of her being able to distinguish between a very light and a very dark chamber, and between a gloomy day and sunshine, rendered it probable that the nerve was in a sound and natural state. Under this impression, I thought that the restoration of her sight by making an artificial pupil was practicable, and certainly well worthy of a trial. Accordingly, on the 26th of January, I introduced a very small needle through the cornea, passing it also through the centre of the iris; but I could not destroy any of the adhesions which had shut up the pupillar opening. After this operation she said she could distinguish more light, but she could perceive neither forms nor colours. The result of this first attempt justified the favourable views entertained of the state of the retina, and Mr. Lawrence, who at this time was consulted, coincided with me in this opinion.

On the 8th of February, a second operation was performed, which consisted in passing a sharp edged needle through the sclerotica, bringing its point through the iris into the anterior chamber, repassing it into the posterior chamber at a small distance, and then dividing the portion of iris thus included between the two perforations of the needle. Only a very slight inflammation followed,—the light became offensive to her,—she complained of its brightness, and was frequently observed trying to see her hands; but it was evident her vision was very imperfect; for although there was an incision made in the iris, some opaque matter lay behind this opening, which must have greatly obstructed the entrance of light.

On the 17th of February, a third operation was performed, which consisted in still further enlarging the opening in the iris, and in removing the opaque matter, by a needle introduced through the sclerotica. This was followed by a very slight degree of redness. The operation being performed at my house, she returned home in a carriage, with her eye covered only with a loose piece of silk, and the first thing she noticed was a hackney coach passing, when she exclaimed, "What is that large thing that has passed by us?" In the course of the evening she requested her brother to show her his watch, concerning which she expressed much curiosity, and she looked at it a considerable time, holding it close to her eye. She was asked what she saw, and she said there was a dark and a bright side; she pointed to the hour of 12, and smiled. Her brother asked her if she saw any thing more? she replied, "Yes," and pointed to the hour of 6, and to the hands of the watch. She then looked at the chain and seals, and observed that one of the seals was bright, which was the case, being a solid piece of rock crystal. The following day I asked her to look again at the watch, which she refused to do, saying, that the light was offensive to her eye, and that she felt very stupid; meaning that she was much confused by the visible world thus for the first time opened to her. On the third day she observed the doors on the opposite side of the street, and asked if they were red, but they were in fact of an oak colour. In the evening she looked at her brother's face, and said that she saw his nose; he asked her to touch it, which she did; he then slipped a handkerchief over his face, and asked her to look again, when she playfully pulled it off, and asked, "What is that?"

On the sixth day, she told us that she saw better than she had done on any preceding day; "but I cannot tell what I do see; I am quite stupid." She seemed indeed bewildered from not being able to combine the knowledge acquired by the senses of touch and sight, and felt disappointed in not having the power of distinguishing at once by her eye, objects which she could so readily distinguish from one another by feeling them.

On the seventh day she took notice of the mistress of the house in which she lodged, and observed that she was tall. She asked what the colour of her gown was? to which she was answered, that it was blue: "so is that thing on your head," she then observed; which was the case: "and your handkerchief, that is a different colour;" which was also correct. She added, "I see you pretty well, I think." The teacups and saucers underwent an examination: "what are they like?" her brother asked her. "I don't know," she replied; "they look very queer to me; but I can tell what they are in a minute when I touch them." She distinguished an orange on the chimney-piece, but could form no notion of what it was till she touched it. She seemed now to have become more cheerful, and entertained greater expectation of comfort from her admission into the visible world; and she was very sanguine that she would find her newly acquired faculty of more use to her when she returned home, where every thing was familiar to her.

On the eighth day, she asked her brother, when at dinner, "what he was helping himself to?" and when she was told it was a glass of port wine, she replied, "port wine is dark, and looks to me very ugly." She observed, when candles were brought into the room, her brother's face in the mirror, as well as that of a lady who was present; she also walked, for the first time without assistance, from her chair to a sopha which was on the opposite side of the room, and back again to the chair. When at tea, she took notice of the tray, observed the shining of the japan work, and asked "what the colour was round the edge?" she was told that it was yellow; upon which she remarked, "I will know that again."

On the ninth day she came down stairs to breakfast in great spirits; she said to her brother, "I see you very well to-day;" and came up to him, and shook hands. She also observed a ticket on a window of a house on the opposite side of the street ("a lodging to let"); and her brother, to convince himself of her seeing it, took her to the window three several times, and to his surprise and gratification, she pointed it out to him distinctly on each trial.

She spent a great part of the eleventh day looking out of the window, and spoke very little.

On the twelfth day she was advised to walk out, which recommendation pleased her much. Mr. —— called on her, and she told him she felt quite happy. Her brother walked out with her as her guide, and took her twice round the piazzas of Covent-garden. She appeared much surprised, but apparently delighted; the clear blue sky first attracted her notice, and she said, "it is the prettiest thing I have ever seen yet, and equally pretty every time I turn round and look at it." She distinguished the street from the foot pavement distinctly, and stepped from one to the other like a person accustomed to the use of her eyes. Her great curiosity, and the manner in which she stared at the variety of objects, and pointed to them, exciting the observation of many by-standers, her brother soon conducted her home, much against her will.

On the thirteenth day nothing particular took place till tea-time, when she observed that there was a different tea-tray, and that it was not a pretty one, but had a dark border; which was a correct description. Her brother asked her to look in the mirror, and tell him if she saw his face in it? to which she answered, evidently disconcerted, "I see my own; let me go away."

She drove in a carriage, on the fourteenth day, four miles on the Wandsworth road; admired most the sky and the fields, noticed the trees, and likewise the river Thames as she crossed Vauxhall bridge. At this time it was bright sunshine, and she said something dazzled her when she looked on the water.

On the fifteenth day, being Sunday, she walked to a chapel at some distance, and now evidently saw more distinctly, but appeared more confused than when her sight was less perfect. The people passing on the pavement startled her; and once when a gentleman was going past her, who had a white waistcoat and a blue coat with yellow buttons, which the sunshine brought full in her view, she started so as to draw her brother, who was walking with her, off the pavement. She distinguished the clergyman moving his hands in the pulpit, and observed that he held something in them; this was a white handkerchief.

She went in a coach, on the sixteenth day, to pay a visit in a distant part of the town, and appeared much entertained with the bustle in the streets. On asking her how she saw on that day? she answered, "I see a great deal, if I could only tell what I do see; but surely I am very stupid."

Nothing particular took place on the seventeenth day; and when her brother asked her how she was? she replied, "I am well, and see better; but don't tease me with too many questions, till I have learned a little better how to make use of my eye. All that I can say is, that I am sure, from what I do see, a great change has taken place; but I cannot describe what I feel."

Eighteen days after the last operation had been performed, I attempted to ascertain by a few experiments her precise notions of the colour, size, forms, position, motions and distances of external objects. As she could only see with one eye, nothing could be ascertained respecting the question of double vision. She evidently saw the difference of colours; that is, she received and was sensible of different impressions from different colours. When pieces of paper one and a half inch square, differently coloured, were presented to her, she not only distinguished them at once from one another, but gave a decided preference to some colours, liking yellow most, and then pale pink. It may be here mentioned, that when desirous of examining an object, she had considerable difficulty in directing her eye to it, and finding out its position, moving her hand as well as her eye in various directions, as a person when blind-folded, or in the dark, gropes with his hands for what he wishes to touch. She also distinguished a large from a small object, when they were both held up before her for comparison. She said she saw different forms in various objects which were shown to her. On asking what she meant by different forms, such as long, round and square, and desiring her to draw with her finger these forms on her other hand, and then presenting to her eye the respective forms, she pointed to them exactly: she not only distinguished small from large objects, but knew what was meant by above and below; to prove which, a figure drawn with ink was placed before her eye, having one end broad, and the other narrow, and she saw the positions as they really were, and not inverted. She could also perceive motions; for when a glass of water was placed on the table before her, on approaching her hand near it, it was moved quickly to a greater distance, upon which she immediately said, "You move it; you take it away."

She seemed to have the greatest difficulty in finding out the distance of any object; for when an object was held close to her eye, she would search for it by stretching her hand far beyond its position, while on other occasions she groped close to her own face, for a thing far removed from her.

She learned with facility the names of the different colours, and two days after the coloured papers had been shown to her, on coming into a room the colour of which was crimson, she observed that it was red. She also observed some pictures hanging on the red wall of the room in which she was sitting, distinguishing several small figures in them, but not knowing what they represented, and admiring the gilt frames. On the same day, she walked round the pond in the centre of St. James's square, and was pleased with the glistening of the sun's rays on the water, as well as with the blue sky and green shrubs, the colours of which she named correctly.

It may be here observed, that she had yet acquired by the use of her sight but very little knowledge of any forms, and was unable to apply the information gained by this new sense, and to compare it with what she had been accustomed to acquire by her sense of touch. When, therefore, the experiment was made of giving her a silver pencil case and a large key to examine with her hands; she discriminated and knew each distinctly; but when they were placed on the table, side by side, though she distinguished each with her eye, yet she could not tell which was the pencil case and which was the key.

Nothing farther occurred in the history of this lady's case worthy of notice till the twenty-fifth day after the operation. On that day she drove in a carriage for an hour in the Regent's Park, and on her way there seemed more amused than usual, and asked more questions about the objects surrounding her, such as "What is that?" it is a soldier, she was answered; "and that, see! see!" these were candles of various colours at a tallow chandler's window. "Who is that, that has passed us just now?" it was a person on horseback: "but what is that on the pavement, red?" it was some ladies who wore red shawls. On going into the Park, she was asked what she saw particularly, or if she could guess what any of the objects were. "Oh yes," she replied, "there is the sky, that is the grass; yonder is water, and two white things;" which were two swans. On coming home along Piccadilly, the jewellers' shops seemed to surprise her much, and her expressions made those around her laugh heartily.

From this period till the time of her leaving London on the 31st of March, being forty-two days after the operation, she continued almost daily to gain more information of the visible world, but she had yet much to learn. She had acquired a pretty accurate notion of colours and their different shades and names; and when she came to pay me a farewell visit, she then wore a gown, the first of her own choice, with the light purple colour of which she seemed highly gratified, as well as with her cap, which was ornamented with red ribbons. She had not yet acquired any thing like an accurate knowledge of distance or of forms, and up to this period she continued to be very much confused with every object at which she looked. Neither was she yet able, without considerable difficulty and numerous fruitless trials, to direct her eye to an object; so that when she attempted to look at any thing, she turned her head in various directions, until her eye caught the object of which it was in search. She still entertained however the same hope which she expressed soon after the operation, that when she got home her knowledge of external things would be more accurate and intelligible, and that when she came to look at those objects which had been so long familiar to her touch, the confusion which the multiplicity of external objects now caused, would in a great measure subside.

May, 1826.

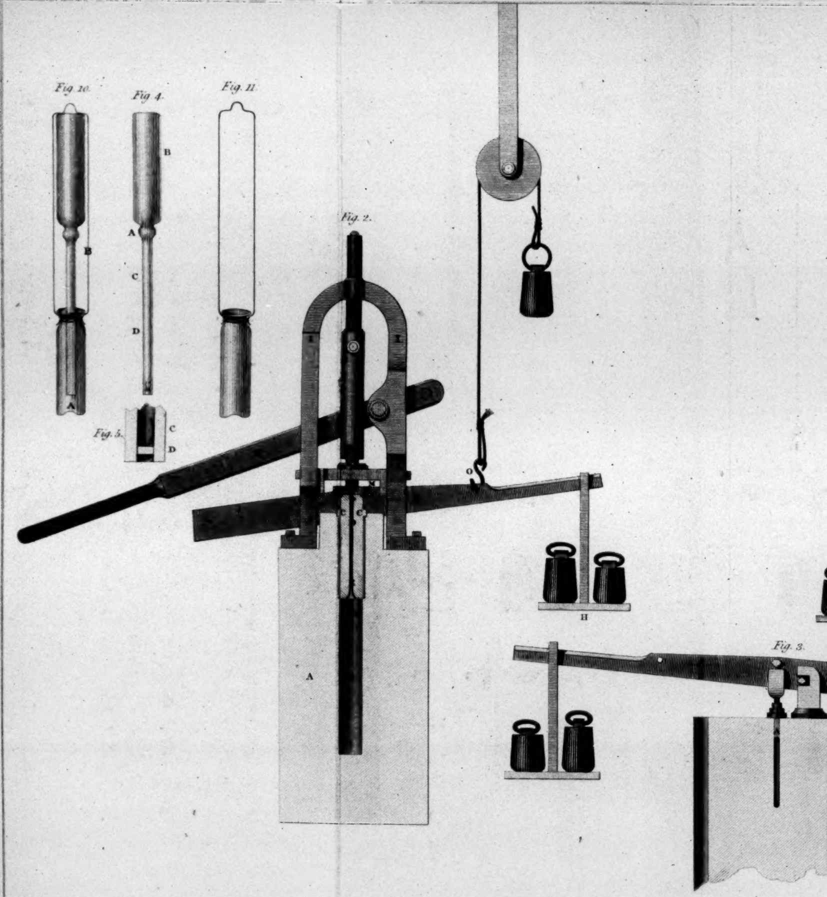

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Fig. 6.

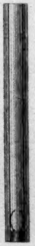
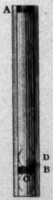


Fig. 7.



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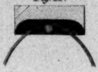

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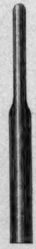


Fig. 8.

Fig. 1.

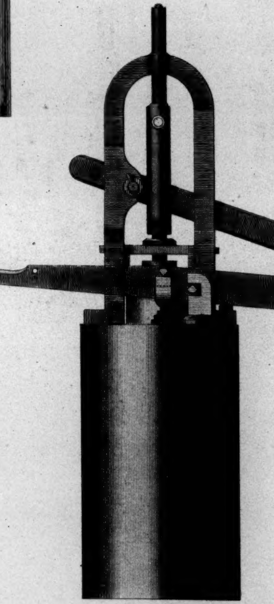
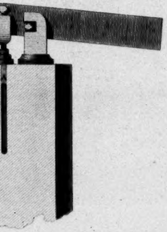


Fig. 3.



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XXXII. *On the progressive compression of water by high degrees of force, with some trials of its effects on other fluids. By J. Perkins. Communicated by* W. H. Wollaston, *M. D. V.P.R.S. May* 25.

Read June 15, 1826.

The apparatus suggested by me in a paper read before the Royal Society the 22nd of June, 1820, on the compressibility of water, having now been completed, I will first describe its construction, and then give an account of the experiments already made with it.

Fig. 1. In the accompanying drawing, Plate XX. is a view of the compressing machine, and fig. 2 is a sectional view of the same, of which the following is a description. A, is a cylinder of gun metal, the length of which is 34 inches, and its external diameter $13\frac{1}{2}$ inches; B, is the receiver of the compressor, being $1\frac{1}{2}$ inch in diameter, and 29 inches long; C, is the barrel of a steel pump, $8\frac{1}{2}$ inches long and $1\frac{1}{4}$ inches diameter, on the outside of which a screw is cut 7 inches in length, by which it is finally united to the compressor; D, is the chamber or caliber of the pump, $\frac{5}{16}$ of an inch in diameter, and continuing of the same size throughout the whole length, excepting a conical enlargement at the bottom and top of the pump; the bottom cone receives the valve B when opening inwards; F, the piston or plunger of steel, most accurately fitted to the bore of the pump, having its lower extremity hollowed out to a semi-elliptical cup, with a very thin edge, the expansion of which during the

descent of the piston occasions it to be water-tight, without need of any stuffing. The lever G, which in this view passes behind the pump, is part of the apparatus for indicating the force of compression. Fig 3, represents a section of the indicator. A, is a cylindrical tube, communicating with the receiver of the compressor, and of such dimensions that its area is equal to $\frac{1}{14}$ of a square inch. Hence the number of pounds pressing on its piston by means of the lever G, indicates directly the number of atmospheres used for compression; since the arms of the lever are in proportion of 10 to 1, and its weight counterpoised by means of the hook O and weight, fig. 2, every pound in the scale represents 10 atmospheres of compressing force.

For the purpose of measuring the diminution of bulk that water undergoes, a glass piezometer, fig. 4, is used for containing it. This consists of an elongated bulb, nearly four inches in length and about $\frac{3}{4}$ of an inch in diameter, with a tube of regular bore, 9 inches long and about $\frac{1}{8}$ of an inch diameter internally. By weighing the quantity of quicksilver contained in this instrument when full, and ascertaining the weight contained in a given length of the tube alone, it was found that the whole content was equal to a tube of 190 inches long, having the diameter of the contracted part of the piezometer. Fig. 5, shows a section of the bottom of the piezometer, containing a small disk of steel D, and above it a delicate hair spring C, of sufficient strength to retain its position after being pressed upwards in the tube, so as to serve as register of the degree of compression that has been effected. Fig. 11, is a phial containing a small quantity of quicksilver, in which the piezometer, full of water, with its register and disk, is to be inverted for experiment, as shown fig. 10.

The piezometer so arranged was then placed in the receiver of the compressor, which was filled with water, and kept at a temperature of 50 degrees Fahrenheit. The steel pump C, was next screwed firmly into its upper end, which is $\frac{1}{2}$ an inch longer than the screw or body of the pump, entering a recess of the cylinder of the compressor, of about $\frac{1}{4}$ of an inch in depth, which it exactly fitted. Between the head of the pump and the bottom of the recess was placed a collar of lead, for the purpose of making a more perfect joint.

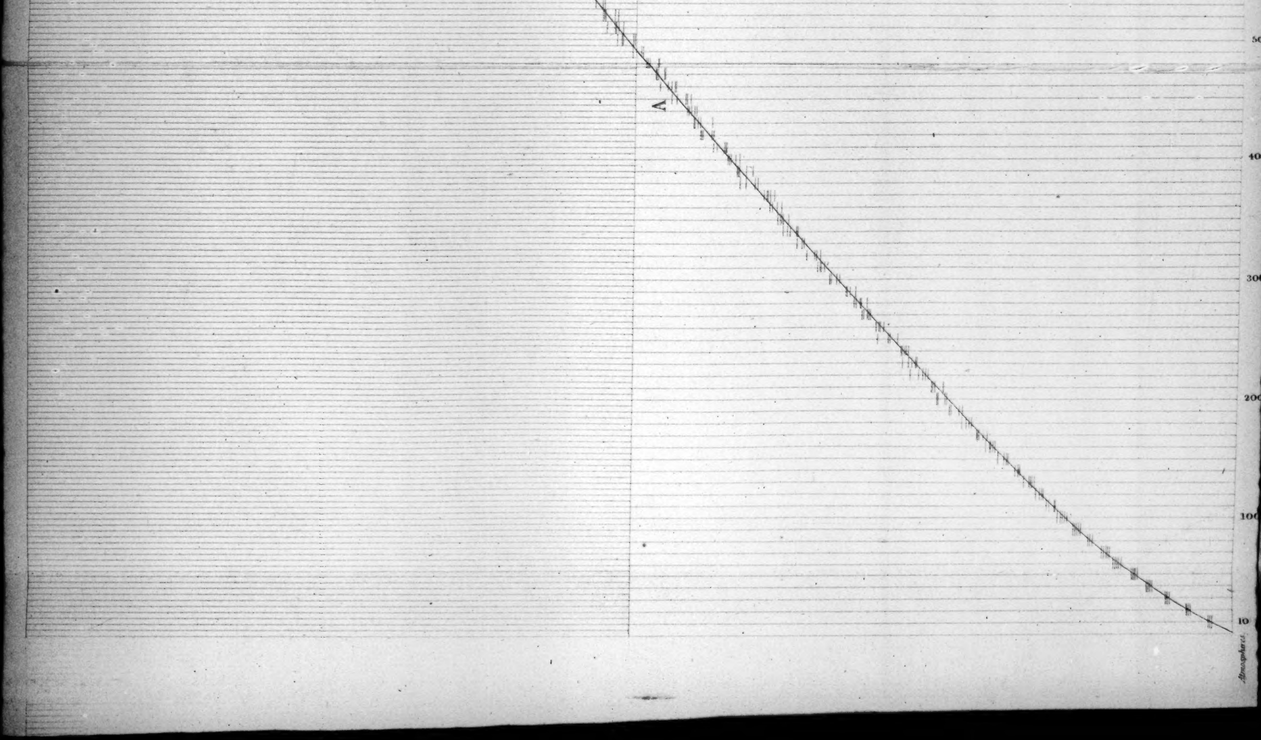
The frame supporting the piston and pump handles was next screwed to the cylinder; about $\frac{1}{2}$ an inch of the upper end of the pump was bell-mouthed, the better to receive the piston, which rises quite out of the pump. Two desirable objects are effected by the piston leaving the chamber at each stroke, *viz.* 1st. It allows the water to fill the pump in the simplest manner; 2nd. The piston at each stroke receives a portion of the oil, which floats on the surface of the water in the cup M, which is of great consequence in high pressure. The piston F, contracts as it enters the bell mouth of the pump, which, together with the pressure of the water against the interior of the piston, causes it to fit as perfectly as the leather cup in the barrel of an air pump. All being ready, the pump was set to work, and as soon as the intended pressure was known to be effected by use of the valve, the pressure was gradually taken off, by unscrewing the pump C. The piezometer being removed from the compressor, the indicating spring was found raised in the tube, more or less, according to the power employed. The greatest amount of which this apparatus admitted was 1000 atmospheres; being equal to 14,000 pounds to the square inch.

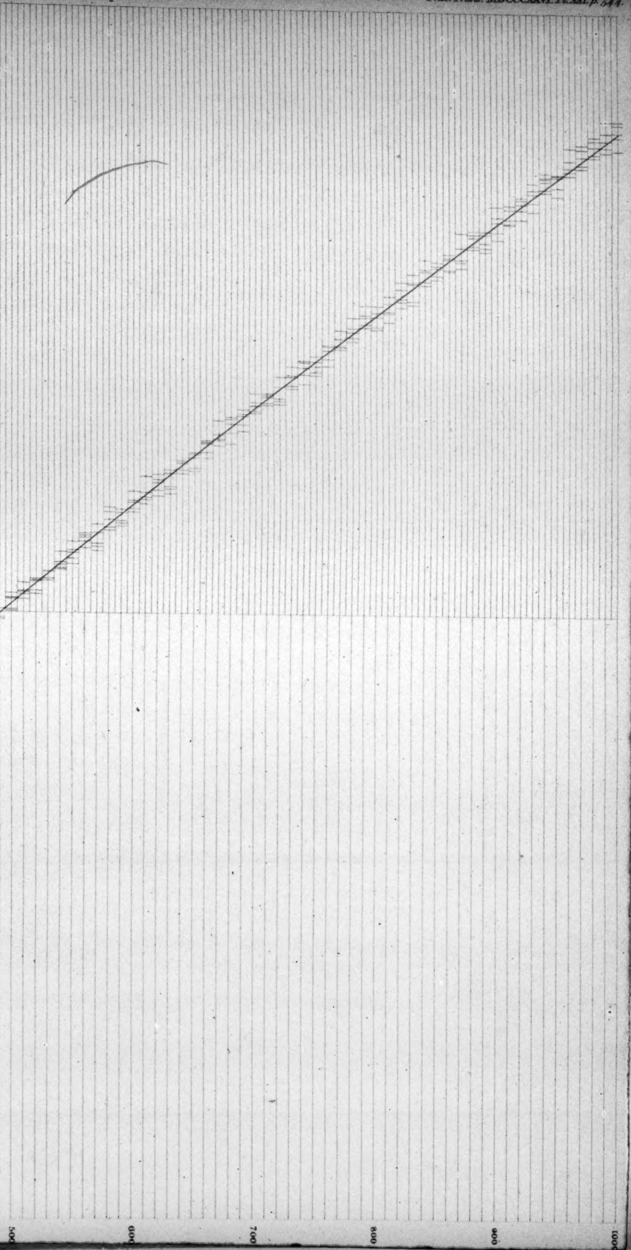
In order to estimate (though less accurately) higher degrees of compression, I had another piezometer, fig. 6, eight inches long, ground internally, perfectly cylindrical, and stopped at its upper extremity, with a flat disk of glass cemented into it. This tube I filled with water, and subjected it to a pressure of 2000 atmospheres. After repeating this experiment a great number of times, the average of the result showed that the column of water, 8 inches long, was compressed $\frac{2}{3}$ of an inch, or $\frac{1}{12}$ part of its length.

For the purpose of representing the law of condensation of water by different degrees of compressing force, I had a plate, 2, engraved, with parallel lines of $\frac{1}{10}$ of an inch a-part, to which the measure taken in each experiment was immediately transferred.

After the results of 5 experiments had been thus laid down for every 10 atmospheres, as far as 1000, a curve line has been drawn through the mean of them, as near as could be done to preserve a regular curve line. It may readily be seen, that there are various irregularities observable; but as the original records remain upon the plate, those who choose to theorize upon the subject, have the means of drawing any other curve that may suit their views of the true law of condensation.

With the same apparatus, I also made experiments on the compression of other fluids. The most remarkable result I obtained was with concentrated acetic acid; which, after compression with a force of 1100 atmospheres, was found to be beautifully crystallized, with the exception of about $\frac{1}{10}$ part of fluid, which, when poured out, was only slightly acid.


A
500
400
300
200
100
10
Atmospheres


500
600
700
800
900
1000

I next applied the apparatus to the compression of aëriform fluids.

A gasometer B, was filled half full of water; it was then inverted, and placed in receiving tube A, as represented in fig. 8: it was found that, under a pressure of 500 atmospheres, the whole of the air was taken up by the water; but none of it was given out when the pressure was taken off.

As it might be supposed that even glass was pervious to water by such a force, a small phial was made air-tight, by fitting into its neck a well-ground glass stopper. It sustained pressure of 500 atmospheres without change, and was perfectly dry within, although it remained under that pressure 15 minutes. It was next subjected to a pressure of 800 atmospheres, and when taken out was found to be crushed to atoms.

In the course of my experiments on the compression of atmospheric air, by the same apparatus which had been used for compressing water, I observed a curious fact, which induced me to extend the experiment; *viz.* that of the air beginning to disappear at a pressure of 500 atmospheres, evidently by partial liquefaction, which is indicated by the quicksilver not settling down to a level with its surface. At an increased pressure of 600 atmospheres, the quicksilver was suspended about $\frac{1}{8}$ of the volume up the tube or gasometer; at 800 atmospheres, it remained about $\frac{1}{3}$ up the tube; at 1000 atmospheres, $\frac{2}{3}$ up the tube, and small globules of liquid began to form about the top of it; at 1200 atmospheres, the quicksilver remained $\frac{3}{4}$ up the tube, and a beautiful transparent liquid was seen on the surface of the quicksilver, in quantity about $\frac{1}{2000}$ part of the column of air. The gasometer, fig. 9, was at another time charged with carburetted hydrogen, and placed in the receiving tube A, fig. 8, with its mouth immersed in the quicksilver C; it was subjected to different pressures, and it began to liquefy at about 40 atmospheres, and at 1200 atmospheres the whole was liquefied.

These instances of apparent condensation of gaseous fluids were first observed in January, 1822; but for want of chymical knowledge requisite to ascertain the exact nature of the liquids produced, I did not pursue the inquiry further; and as the subject has been taken up by those who are eminently qualified for the investigation, I need not regret my inability to make full advantage of the power I had the means of applying.

I have been desirous of ascertaining the law of condensation of gaseous fluids, at the high degrees of pressure; but as the apparatus for the compression of water, which requires to be made sensible at its lower extremity, is by no means adapted to show the higher degrees of compression of gases, a suitable apparatus requires to be made for such experiments; which I shall hope to make the subject of a future communication.

This Table shows, in inches and parts, the compression of a column of 190 inches of water, corresponding to every 10 atmospheres to 1000 inclusive.

Atmos.	Inches.	Inches.	Inches.	Inches.	Inches.	Atmos.	Inches.	Inches.	Inches.	Inches.	Inches.
10	0.176	0.191	0.200			510	5.110	5,147	5.170	5.180	5.241
20	0.350	0.367	0.380	0.390		520	5.150	5.270	5.255	5.267	5.280
30	0.522	0.540	0.550	0.560		530	5.260	5.275	5.340		
40	0.665	0.680	0.693	0.700	0.715	540	5.355	5.363	5.393	5.413	5.500
50	0.784	0.800	0.810	0.825	0.840	550	5.397	5.443	5.491	5.513	5.587
60	0.927	0.943	0.950	0.970	0.991	560	5.530	5.583	5.620	5.635	5.653
70	1.026	1.040	1.055	1.067	1.090	570	5.510	5.545	5.570	5.650	5.730
80	1.170	1.190	1.200			580	5.680	5.700	5.765	5.830	5.865
90	1.265	1.285	1.300	1.327	1.333	590	5.710	5.737	5.751	5.820	5.850
100	1.385	1.400	1.420	1.440	1.465	600	5.831	5.860	5.913	5.930	6.000
110	1.485	1.495	1.537	1.560		610	5.897	5.940	5.955	5.991	6.120
120	1.585	1.595	1.617	1.647		620	5.960	6.013	6.070	6.100	6.140
130	1.650	1.670	1.685	1.700	1.745	630	5.977	6.020	6.040	6.145	6.171
140	1.780	1.790	1.800	1.830		640	6.150	6.186	6.230	6.247	6.258
150	1.880	1.893	1.915	1.967		650	6.170	6.193	6.280	6.310	6.325
160	1.990	2.010	2.025	2.040	2.070	660	6.280	6.330	6.397	6.410	6.423
170	2.050	2.090	2.120	2.140	2.150	670	6.383	6.421	6.440	6.470	6.590
180	2.190	2.200	2.233	2.267		680	6.400	6.491	6.563	6.600	6.620
190	2.267	2.291	2.360	2.370		690	6.500	6.555	6.627	6.643	6.681
200	2.395	2.413	2.470	2.480		700	6.653	6.670	6.713	6.747	6.813
210	2.427	2.495	2.515	2 530		710	6.720	6.740	6.781	6.795	6.813
220	2.550	2.570	2.593	2.630	2.700	720	6.725	6.750	6.770	6.870	6.950
230	2.643	2.650	2.687	2.710	2.763	730	6.855	6.880	6.951	6.965	7.040
240	2.715	2.740	2.750	2.770	2.783	740	6.900	6.980	7.037	7.080	7.093
250	2.800	2.870	2.890	2.977		750	6.961	6.991	7.040	7.080	7.121
260	2.923	2.953	2.970	2.990		760	7.050	7.110	7.177	7.211	7.224
270	3.035	3.050	3.060	3.090	3.100	770	7.187	7.29	7.216	7.275	7.343
280	3.060	3.110	3.120	3.150	3.170	780	7.247	7.280	7.320	7.350	7.450
290	3.157	3.205	3.230	3.240		790	7.370	7.400	7.450	7.470	5.550
300	2.297	3.320	3.365	3.373		800	7.350	7.375	7.450	7.491	7.445
310	3.420	3 450	3.460	3.480		810	7.410	7.480	7.497	7.570	7.627
320	3.450	3.490	3.500	3.567		820	7.540	7.587	7.610	7.690	7.745
330	3.595	3.615	3.653			830	7.563	7.590	7.737	7.745	7.815
340	3.641	3.650	3.710	3.733	3.770	840	7.723	7.760	7.840	7.857	7.940
350	3.720	3 767	3.780	3 791	3.813	850	7.775	7,810	7.867	7.885	7.920
360	3.785	3.823	3.863	3 887		860	7.843	7.860	7.943	7.955	7.990
370	3.847	3.880	3 895	3.910	3.933	872	7.870	7.920	7.940	8,033	8,160
380	3.980	4.000	4.800	4.129		880	8.027	8.050	8.130	8.150	8.180
390	4.013	4.080	4.140	4.150	4.159	890	8.500	8 040	8,080	8.147	8.173
400	4.133	4.170	4 210	4.220	4.231	900	8.110	8.170	8.260	8.290	8.385
410	4.245	4.253	4.263	4.320	4.350	910	8.237	8.247	8.277	8.360	8.393
420	4.353	4.365	4.445	4.450	4.456	920	8.377	8.350	8.400	8.443	8.460
430	4.360	4.460	4.480	4.570	4.518	930	8.327	8.420	8.490	8.525	8.580
440	4.490	4.500	4.530	4.555	4.565	940	8.500	8,540	8.590	8.627	8.640 & 8.670
450	4.543	4.570	4.585	4.653	4.700	950	8.480	8.550	8.637	8.650	8.660
460	4.660	4.670	4.700	4.730	4.795	960	8.650	8 680	8.710	8.767	8.880
470	4.753	4.760	4.813	4.827	4.833	970	8.710	8.735	8.800	8.870	8.900
480	4 810	4.815	4.880	4.900	4.910	980	8.800	8.825	8.890	8.940	8.970
490	4.950	4.963	5 010	5.040		990	8.847	8.880	8.938	9.000	9.100
500	5.010	5.035	5.110	5.120	5.160	1000	8.855	8.973	9.005	9.076	9.100

It will appear, that at some pressures the result of only three or four experiments have been tabulated; this apparent omission arises from the entire coincidence of two or more of the experiments, and from my having omitted at the time to notice which were those which so agreed.

XXXIII. *On the figure of the earth.* By GEORGE BIDDELL AIRY, *M.A. Fellow of Trinity College, Cambridge. Communicated by* J. F. W. HERSCHEL, *Esq. Sec. R. S.*

Read June 15, 1826.

THE ellipticity of the earth, deduced by Captain SABINE from a series of pendulum experiments the most extensive, and apparently the most deserving of confidence, that has ever been made, differs considerably from that which, as is generally believed, is indicated by geodetic measures. The difference can only be explained by errors of observation, by peculiarities of local circumstances, or by some defect in the theory which connects the figure of the earth with the variation of gravity on its surface: under the last head may be placed defects in the mathematical part of the theory, and errors in the assumptions of the original constitution and present state of the earth. It was with a view to ascertain the sufficiency of the mathematical theory, that I undertook the investigations contained in this paper. The celebrated proposition called CLAIRAUT's theorem, by which the earth's ellipticity is inferred from the variation of gravity on its surface, is obtained only by the rejection of the squares and higher powers of the ellipticity. It is by the same rejection that the figure of the earth, supposed a heterogeneous fluid, is proved to be an elliptic spheroid. It appeared therefore probable, that a more accurate theory might introduce some modification into CLAIRAUT's theorem, and might also show the figure of the earth to differ from an ellipsoid; and there was no reason to think that the first approximation to that figure was more accurate, than the first approximation to the motion of the moon's perigee. The result of my investigation does not at all serve to reconcile the pendulum observations made by Captain SABINE with the measures of degrees; and with respect to one object, which I hoped to obtain, I am therefore completely unsuccessful. The theory shows, however, that the earth's figure, on the usual suppositions as to its constitution, is not an elliptic spheroid; and the formulæ which I have obtained will give the means of determining very exactly the figure of the earth, when the experiments on the variation of gravity, or the measures of arcs on the earth's surface, shall be thought sufficiently accurate. As the subject is one whose interest is not confined to the present time, I have ventured to offer my investigations to the Royal Society.

The first part of the following sheets contains the theory of the heterogeneous earth, pushed so far as to include all the terms of the second order: it is succeeded by a comparison of this theory with Captain SABINE's results, and with the best arcs of the meridian that have been measured: and in the conclusion, I have offered some suggestions on the propriety of repeating some of these measures.

(1.) To ascertain the form of equilibrium of a fluid, it is first necessary to find the sum of the products of each particle by the reciprocal of its distance from any point of the fluid (LAPLACE, *Mecanique Celeste*, livre iii. n°. 23). This can be done only by assuming a function with indeterminate constants to represent the form of each stratum of equal density: then finding that sum, and applying to it the conditions of equilibrium, the values of the constants will be found. As we propose to carry our approximation to the second order of the difference from a sphere, or the second order of the ratio of the centrifugal force to the force of gravity, it is evident that, without something to guide us, this will be a work of considerable labour.

(2.) Here, however, we shall derive some assistance from former investigations. CLAIRAUT and LAPLACE have shown that, to the first order, the form of every surface of equal density is an elliptic spheroid: the difference, consequently, of any surface of equal density from an elliptic spheroid is only of the second order. If a be the polar semi-axis of an elliptic spheroid, $a(1+e)$ the equatorial semi-axis, μ' the sine of the latitude of any point, (the latitude being that which is usually termed the corrected latitude), R the radius drawn to that point, then $R = a\left(1 + e.\overline{1-\mu'^2} - \frac{3e^2}{2}\overline{\mu'^2-\mu'^4}\right)$. The radius then of a surface of equal density is $a\left(1 + e.\overline{1-\mu'^2} - \frac{3e^2}{2}.\overline{\mu'^2-\mu'^4}\right) +$ a quantity of the second order. Now, upon using the elliptic value of R, it would be found that the equation of equilibrium could not be satisfied, in consequence of the appearance of μ'^4: but no higher powers of μ' would enter into that equation. To enable us to take away these terms, R must be increased by a function of μ', containing none but the even powers of μ' as far as μ'^4. The most convenient form that we can take is $a.A(\mu'^4-\mu'^2)$, since it vanishes both at the pole and at the equator, and at middle latitudes expresses the depression of the surface below the ellipsoid whose axes are the same. The value of R then in the spheroidal surface of equal density is assumed to be $a\left(1 + e.\overline{1-\mu'^2} - \left(\frac{3e^2}{2} + A\right).\overline{\mu'^2-\mu'^4}\right)$.

(3.) Let V be the sum of the products of each particle by the reciprocal of its distance from any point: let μ be the sine of the latitude of that point, r its distance from the centre: also let θ and ω be the latitude and longitude of that point, θ' and ω' those of any other point. Then it is easily found that their distance $= \sqrt{\{r^2 - 2r\text{R}(\sin\theta.\sin\theta' + \cos\theta.\cos\theta'.\cos\overline{\omega'-\omega}) + \text{R}^2\}} = \sqrt{\{r^2 - 2r\text{R}(\mu\mu' + \sqrt{1-\mu^2}\sqrt{1-\mu'^2}.\cos\overline{\omega'-\omega}) + \text{R}^2\}}$: let this $= z$. Suppose now the heterogeneous spheroid divided into wedges by planes passing through the axis, and suppose each of these wedges divided into pyramids whose vertices are at the centre: let $\delta\omega'$ be the angle of two planes, and $\delta\theta'$ the angle made by the two surfaces of a pyramid which cut these planes; and suppose the pyramid divided into frustra, the length of one being δR. Then the solid content of this frustrum is ultimately $\text{R}\,\delta\theta'.\text{R}\cos\theta'\delta\omega'.\delta\text{R} = \text{R}^2\delta\text{R}.\delta\omega'.\delta\mu'$ ultimately; and if ρ be its density, the product of the particle into the reciprocal of its distance from the given point is ultimately $\frac{\rho.\text{R}^2\delta\text{R}.\delta\omega'.\delta\mu'}{z}$. Consequently, to find the sum of those products for the spheroid, we must integrate $\frac{\rho\text{R}^2}{z}$ with respect to R, ω', and μ': or, which amounts to the same, we must integrate $\frac{\rho\text{R}^2}{z}.\frac{d\text{R}}{da}$ with respect to a, ω', and μ': that is, we must take $\int_a\int_{\omega'}\int_{\mu'}\frac{\rho\text{R}^2}{z}.\frac{d\text{R}}{da}$[*], or in the common notation $\iiint \frac{\rho R^2}{z} . \frac{dR}{da} . da . d\omega' . d\mu'$. The limits of μ' are -1 and $+1$: those of ω are o and 2π: and those of a are 0, and the value of a at the external surface, which we shall call a.

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[*] I prefer this notation, as it does not necessarily carry with it the idea of infinitely small quantities.

(4.) To perform these integrations, it is necessary to expand the expression for z in a series of powers of $\frac{R}{r}$ or $\frac{r}{R}$: the former of course must be used for the strata interior to the point in question, and the latter for the strata exterior to that point, that the series may in both cases converge. Suppose then $\frac{1}{\sqrt{\left\{r^2 - 2rR\left(\mu\mu' + \sqrt{1-\mu^2}\sqrt{1-\mu'^2}\cos\left(\overline{\omega'-\omega}\right) + R^2\right\}\right.}} = \frac{Q^{(0)}}{r} + Q^{(1)} . \frac{R}{r^2} + Q^{(2)} . \frac{R^2}{r^3} + Q^{(3)} . \frac{R^3}{r^4} +$ &c. for the former case, or $= \frac{Q^{(0)}}{R} + Q^{(1)} . \frac{r}{R^2} + Q^{(2)} . \frac{r^2}{R^3} + Q^{(3)} . \frac{r^3}{R^4} +$ &c. for the latter, ($Q^{(0)}$, $Q^{(1)}$, $Q^{(2)}$, &c. being the same in both series): then $Q^{(i)}$ satisfies the following equation. (liv. iii. n°. 9)

$$0 = \frac{d}{d\mu}\left\{\overline{1-\mu^2} . \frac{dQ^{(i)}}{d\mu}\right\} + \frac{1}{1-\mu^2} . \frac{d^2 Q^{(i)}}{d\omega^2} + i . \overline{i+1} . Q^{(i)}$$

which is likewise true if we put μ' for μ and ω' for ω. And the value of V now depends on the integral

$$\frac{1}{r^{i+1}}\int_a\int_{\omega'}\int_{\mu'} \rho . Q^{(i)} . R^{i+2} . \frac{dR}{da} \text{ or } \frac{1}{r^{i+1}}\int_a \rho \int_{\omega'}\int_{\mu'} Q^{(i)} . R^{i+2} . \frac{dR}{da}$$

(since ρ is a function of a alone), for the interior strata, i being made successively $= 0, 1, 2, 3,$ &c.; and on the integral $r^i\int_a \rho \int_{\omega'}\int_{\mu'} \frac{Q^{(i)}}{R^{i-1}} . \frac{dR}{da}$, for the exterior strata, i being made $= 0, 1, 2, 3,$ &c.

(5.) Suppose now R^{i+3} reduced into a series of the form

$B_0 Z'^{(0)} + B_1 Z'^{(1)} + B_2 Z'^{(2)} +$ &c., where B_0, &c. are functions of a, and $Z'^{(k)}$ is a function of μ' that satisfies the equation

$$0 = \frac{d}{d\mu'}\left\{\overline{1-\mu'^2}.\frac{dZ'^{(k)}}{d\mu'}\right\} + k.\overline{k+1}.\,Z.^{(k)}$$

This equation is exactly similar to the equation above for $Q^{(i)}$, since R does not depend on $\omega'-\omega$, and therefore $\frac{d.^2Z^{(k)}}{d\omega'^2}$ is $= 0$. Then $R^{i+2}.\frac{dR}{da} = \frac{1}{i+3}.\frac{d.R^{i+3}}{da} = \frac{1}{i+3}.\frac{d}{da}\left(B_0 Z'^{(0)} + B_1 Z'^{(1)} + B_2 Z'^{(2)} + \&c.\right)$, the general term of which is $\frac{1}{i+3}.\frac{d.B_k}{da}.Z'^{(k)}$ The value of V then for the interior strata is the sum of all the quantities $\frac{1}{(i+3).r^{i+1}}\int_a \rho\frac{d.B_k}{da}\int_{\omega'}\int_{\mu'} Q^{(i)}.\,Z'^{(k)}$ upon giving to i and k the values 0, 1, 2, 3, &c.; and the integral with respect to a must be taken from $a = 0$ to $a =$ the value which it has in the stratum of equal density passing through the given point, which we shall call α.

(6.) For the strata exterior to the given point we must take the sum of the different values of $r^i\int_a \rho\int_{\omega'}\int_{\mu'}\frac{Q^{(i)}}{R^{i-1}}.\frac{dR}{da}$. Now $\frac{1}{R^{i-1}}.\frac{dR}{da} = \frac{1}{2-i}.\frac{d}{da}.\frac{1}{R^{i-2}}$, except $i = 2$, when $\frac{1}{R}.\frac{dR}{da} = \frac{d}{da}.\log.R$. Suppose then $\frac{1}{R^{i-2}}$ or R^{2-i} expanded into the series $b_0 z'^{(0)} + b_1 z'^{(1)} + b_2 z'^{(2)}$, &c. where $z'^{(k)}$ satisfies the same equation as before; then for the value of V we must take the sum of all the quantities $\frac{r^i}{2-i}\int_a \rho\frac{d.b_k}{da}.\int_{\omega'}\int_{\mu'} Q^{(i)}.z'^{(k)}$, upon giving to k the values

0, 1, 2, 3, &c. and to i the values 0, 1, 3, 4, &c. For the value $i = 2$, we must suppose log. R expanded into the series $c_0 y'^{(0)} + c_1 y'^{(1)} + c_2 y'^{(2)} +$ &c. and must take the sum of the quantities $r^2 \int_a \rho \frac{d . c_k}{d a} \int_{\omega'} \int_{\mu'} Q^{(2)} . y'^{(k)}$, making k successively $= 0, 1, 2,$ &c. The integrals with respect to a must be taken from $a = \alpha$ to $a = \mathrm{a}$.

(7.) Now LAPLACE has shown (*Mec. Cel.* liv. 3. n°. 12). that $\int_{\omega'} \int_{\mu'} Q^{(i)} . Z'^{(k)}$ from $\mu' = -1$ to $\mu' = +1$, and from $\omega' = 0$ to $\omega' = 2\pi$, will always be $= 0$, except $k = i$. And it appears also (liv. 3. n°. 11.) that $U^{(i)} = \frac{4 \alpha \pi}{2i+1} a^{i+3} . Y^{(i)}$; but $U^{(i)}$ (see n°. 17.) is there $= \alpha . a^{i+3} \int_{\omega'} \int_{\mu'} Q^{(i)} . Y'^{(i)}$ between the same limits, $Y^{(i)}$ being the value of $Y'^{(i)}$ when μ and ω are put for μ' and ω': hence $\int_{\omega'} \int_{\mu'} Q^{(i)} . Y'^{(i)} = \frac{4\pi}{2i+1} Y^{(i)}$, where $Y'^{(i)}$ is any function of μ' and ω' satisfying the equation $0 = \frac{d}{d\mu'} \left\{ \overline{1 - \mu'^2} . \frac{d . Y'^{(i)}}{d\mu'} \right\} + \frac{1}{1 - \mu'^2} . \frac{d^2 Y'^{(i)}}{d\omega'^2} + i . \overline{i+1} . Y'^{(i)}$. Consequently $\int_{\omega'} \int_{\mu'} Q^{(i)} . Z'^{(i)} = \frac{4\pi}{2i+1} Z^{(i)}$: $\int_{\omega'} \int_{\mu'} Q^{(i)} z'^{(i)} = \frac{4\pi}{2i+1} z^{(i)}$; and $\int_{\omega'} \int_{\mu'} Q^{(2)} . y'^{(2)} = \frac{4\pi}{5} y^{(2)}$ where $Z^{(i)}$, $z^{(i)}$, and $y^{(2)}$ are the values of $Z'^{(i)}$, $z'^{(i)}$, and $y'^{(2)}$, when μ' is changed to μ.

(8.) Since we propose to include only the second order of e, and since to that order no powers of μ' beyond the fourth are found in R or any function of R, it follows, that $Q^{(i)} Z'^{(4)}$ and $Q^{(i)} z'^{(4)}$ will be the last terms to be integrated, and therefore i will not exceed 4. And as even powers only of μ' are found in R, there will be no terms as $Z'^{(i)}$ in which i is odd, and therefore it is not necessary to consider any but the even values of i. Now for the interior spheroids, when $i=0$,

$$R^{i+3}=R^3=a^3\left\{1+3e.\overline{1-\mu'^2}+3e^2.\overline{1-\mu'^2}|^2-\left(3A+\frac{9e^2}{2}\right).\overline{\mu'^2-\mu'^4}\right\}:$$

and since by LAPLACE's formula (liv. 3. n°. 16), $Z'^{(i)}$, when it does not depend on ω', is a multiple of $\mu'^i-\frac{i.\overline{i-1}}{2.\overline{2i-1}}.\mu'^{i-2}+\frac{i.\overline{i-1}.\overline{i-2}.\overline{i-3}}{2.4.\overline{2i-1}.\overline{2i-3}}\mu'^{i-4}-$ &c. we must resolve R^3 into multiples of $\mu'^4-\frac{6}{7}\mu'^2+\frac{3}{35}$, of $\mu'^2-\frac{1}{3}$, and constants. Thus we have $R^3=$

$$a^3\left(1+2e+e^2-\frac{2}{5}A\right)-a^3\left(3e+\frac{57}{14}e^2+\frac{3}{7}A\right)\left(\mu'^2-\frac{1}{3}\right)+\left(\frac{15}{2}e^2+3A\right).\left(\mu'^4-\frac{6}{7}\mu'^2+\frac{3}{35}\right)$$

whence $B_0=a^3\left(1+2e+e^2-\frac{2}{5}A\right)$, $Z^{(0)}=1$.

Similarly when $i=2$, $R^5=$

a constant $-a^5\left(5e+\frac{25}{2}e^2+\frac{5}{7}A\right)\left(\mu'^2-\frac{1}{3}\right)+$ a multiple of $\left(\mu'^4-\frac{6}{7}\mu'^2+\frac{3}{35}\right)$:

whence $B_2=-a^5\left(5e+\frac{25}{2}e^2+\frac{5}{7}A\right)$, $Z^{(2)}=\mu^2-\frac{1}{3}$.

And when $i=4$, $R^7=$

a constant $+$ a multiple of $\overline{\mu'^2-\frac{1}{3}}+a^7\left(\frac{63}{2}e^2+7A\right).\left(\mu'^4-\frac{6}{7}\mu'^2+\frac{3}{35}\right)$:

whence $B_4=a^7\left(\frac{63}{2}e^2+7A\right)$, $Z^{(4)}=\mu^4-\frac{6}{7}\mu^2+\frac{3}{35}$.

(9.) Collecting then the different terms of $\frac{1}{(i+3)r^{i+1}}\int_\alpha\rho\frac{dB_k}{da}\int_{\omega'}\int_{\mu'}Q^{(i)}.Z'^{(k)}$, which by (7) has no value except $k=i$, in which case $\int_{\omega'}\int_{\mu'}Q^{(i)}.Z'^{(k)}=\frac{4\pi}{2i+1}Z^{(i)}$, we have V for the interior strata $= \frac{4\pi}{1 \cdot 3 \cdot r} \int_a \rho \frac{d \cdot a^3 \left(1 + 2e + e^2 - \frac{2}{5} A\right)}{da}$

$$- \frac{4\pi}{5 \cdot 5 \cdot r^3} \int_a \rho \cdot \frac{d \cdot a^5 \left(5e + \frac{25}{2} e^2 + \frac{5}{7} A\right)}{da} \left(\mu^2 - \frac{1}{3}\right) + \frac{4\pi}{9 \cdot 7 \cdot r^5} \int_a \rho \frac{d \cdot a^7 \left(\frac{63}{2} e^2 + 7A\right)}{da} \left(\mu^4 - \frac{6}{7} \mu^2 + \frac{3}{35}\right).$$

Let $\int_a \rho \frac{d \cdot a^3 (1 + 2e + e^2 - \frac{2}{5} A)}{da} = \varphi(a)$; $\int_a \rho \frac{d \cdot a^5 (e + \frac{5}{2} e^2 + \frac{1}{7} A)}{da} =$ $\psi(a)$; $\int_a \rho \frac{d \cdot a^7 (e^2 + \frac{2}{9} A)}{da} = \upsilon(a)$, the integrals being made to vanish when $a = 0$: taking them to $a = \alpha$, V for the interior strata $= \frac{4\pi}{3} \left\{ \frac{\varphi(\alpha)}{r} - \frac{3}{5} \cdot \frac{\psi(\alpha)}{r^3} \cdot \overline{\mu^2 - \frac{1}{3}} + \frac{3}{2} \cdot \frac{\upsilon(\alpha)}{r^5} \cdot \overline{\mu^4 - \frac{6}{7} \mu^2 + \frac{3}{35}} \right\}$

(10). For the strata exterior to the point in question, when $i = 0$, $R^{2-i} = R^2$

$= a^2 (1 + 2e \cdot \overline{1 - \mu'^2} + e^2 \cdot \overline{1 - \mu'^2}|^2 - (3e^2 + 2A) \cdot \overline{\mu'^2 - \mu'^4})$

$= a^2 (1 + \frac{4e}{3} + \frac{2e^2}{15} - \frac{4}{15} A) - a^2 (2e + \frac{11}{7} e^2 + \frac{2}{7} A) \cdot \overline{\mu'^2 - \frac{1}{3}}$

$+ a^2 (4e^2 + 2A) \cdot \overline{\mu'^4 - \frac{6}{7} \mu'^2 + \frac{3}{35}}$;

whence $b_0 = a^2 (1 + \frac{4e}{3} + \frac{2e^2}{15} - \frac{4}{15} A)$, $z^{(0)} = 1$.

When $i = 2$, log R

$= \log a + e \cdot \overline{1 - \mu'^2} - \frac{e^2}{2} \cdot \overline{1 - \mu'^2}|^2 - \left(\frac{3e^2}{2} + A\right) \cdot \overline{\mu'^2 - \mu'^4}$

$=$ a constant term $- \left(e - \frac{5}{14} e^2 + \frac{A}{7}\right) \cdot \overline{\mu'^2 - \frac{1}{3}}$

$+$ a multiple of $\mu'^4 - \frac{6}{7} \mu'^2 + \frac{3}{35}$;

whence $c_2 = - \left(e - \frac{5}{14} e^2 + \frac{A}{7}\right)$, $y^{(2)} = \mu^2 - \frac{1}{3}$.

When $i = 4$, $R^{2-i} = \frac{1}{R^2}$

$= \frac{1}{a^2} \left(1 - 2e \cdot \overline{1 - \mu'^2} + 3e^2 \cdot \overline{1 - \mu'^2}|^2 + (3e^2 + 2A) \cdot \overline{\mu'^2 - \mu'^4}\right)$

$=$ a constant $+$ a multiple of $\overline{\mu'^2 - \frac{1}{3}} - \frac{2A}{a^2} \cdot \overline{\mu'^4 - \frac{6}{7} \mu'^2 + \frac{3}{35}}$

whence $b_4 = - \frac{2A}{a^2}$, $z^{(4)} = \mu^4 - \frac{6}{7} \mu^2 + \frac{3}{35}$.

(11). Collecting the terms now in the same manner as in (9), according to the directions in (6), and observing the values of the integrals in (7), we find that part of V which depends on the exterior strata $= \frac{4\pi}{2.1}\int_a \rho \frac{d.a^2(1+\frac{4e}{3}+\frac{2e^2}{15}-\frac{4}{15}A)}{da} -$

$$\frac{4\pi}{5}r^2\int_a \rho \frac{d(e-\frac{5}{14}e^2+\frac{A}{7})}{da}\cdot\overline{\mu^2-\frac{1}{3}}+\frac{4\pi}{2.9}r^4\int_a \rho\cdot\frac{d\frac{A}{a^2}}{da}.\overline{\mu^4-\frac{6}{7}\mu^2+\frac{3}{35}}.$$

Let $\int_a \rho \frac{d.a^2(1+\frac{4e}{3}+\frac{2e^2}{15}-\frac{4}{15}A)}{da}=\tau(a)$; $\int_a \rho \frac{d(e-\frac{5}{14}e^2+\frac{A}{7})}{da}=\chi(a)$; $\int_a \rho\cdot\frac{d}{da}\cdot\frac{A}{a^2}=\sigma(a)$; then taking the integrals from $a=\alpha$ to $a=\mathrm{a}$ we have for the exterior strata,

$$\mathrm{V}=\frac{4\pi}{3}\left\{\frac{3}{2}\left\{\tau(\mathrm{a})-\tau(\alpha)\right\}-\frac{3}{5}r^2\left\{\chi(\mathrm{a})-\chi(\alpha)\right\}\overline{\mu^2-\frac{1}{3}}\right.$$
$$\left.+\frac{r^4}{3}\left\{\sigma(\mathrm{a})-\sigma(\alpha)\right\}\overline{\mu^4-\frac{6}{7}\mu^2+\frac{3}{35}}\right\}.$$

(12). Adding together the expressions in (9) and (11,) we find for the complete value of V, $\frac{4\pi}{3}\left\{\frac{\varphi(\alpha)}{r}+\frac{3}{2}\left\{\tau(\mathrm{a})-\tau(\alpha)\right\}+\left(\frac{\psi(\alpha)}{r^3}+r^2\left\{\chi(\mathrm{a})-\chi(\alpha)\right\}\right).\frac{1-3\mu^2}{5}+\left(\frac{v(\alpha)}{r^5}+\frac{2}{9}r^4\left\{\sigma(\mathrm{a})-\sigma(\alpha)\right\}\right).\left(\frac{9}{70}-\frac{9}{7}\mu^2+\frac{3}{2}\mu^4\right)\right\}$

(13.) When a fluid is in equilibrium,* if x, y, and z, be the co-ordinates of a point in it, and P, Q, R, the forces in those directions, then at a surface of equal density $\mathrm{P}\,\delta x+\mathrm{Q}\,\delta y+\mathrm{R}\,\delta z=0$ (liv. i. nº. 17); and the equilibrium is impossible except $\mathrm{P}\,\delta x+\mathrm{Q}\,\delta y+\mathrm{R}\,\delta z$ be the complete variation of some function U. The equation then to a surface of equal density is $\mathrm{U}=\mathrm{C}$. In the present instance, if the ordinates be measured from the centre, and x be the axis of revolution, and T the time of revolution, the only force besides the attraction is the centrifugal force; the resolved part of which in the direction of x is o, that in the direction of y is $\frac{4\pi^2}{T^2}y$, that in the direction of z is $\frac{4\pi^2}{T^2}z$. This contributes to δU the terms $\frac{4\pi^2}{T^2}(y\delta y + z\delta z)$, and to U the terms $\frac{2\pi^2}{T^2}(y^2 + z^2) = \frac{4\pi}{3} \cdot \frac{3\pi}{2T^2} r^2 \cdot \overline{1-\mu^2}$. And if δM be any attracting particle whose co-ordinates are x', y', z', its attractions in those directions are

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* I have not considered the second condition of equilibrium, given by Mr. IVORY in the Philosophical Transactions for 1824, as the reasoning upon which that Gentleman has founded the necessity of such a condition, appears to me altogether defective.

$$\frac{(x'-x)\,\delta M}{\left(\overline{x'-x}|^2 + \overline{y'-y}|^2 + \overline{z'-z}|^2\right)^{\frac{3}{2}}},\quad \frac{(y'-y)\,\delta M}{\left(\overline{x'-x}|^2 + \overline{y'-y}|^2 + \overline{z'-z}|^2\right)^{\frac{3}{2}}},$$

$\frac{(z'-z)\,\delta M}{\left(\overline{x'-x}|^2 + \overline{y'-y}|^2 + \overline{z'-z}|^2\right)^{\frac{3}{2}}}$: and consequently it contributes to δU the term $\delta M \frac{(x'-x)\delta x + (y'-y)\delta y + (z'-z)\delta z}{\left(\overline{x'-x}|^2 + \overline{y'-y}|^2 + \overline{z'-z}|^2\right)^{\frac{3}{2}}}$, and to U the term $\frac{\delta M}{\sqrt{\left(\overline{x'-x}|^2 + \overline{y'-y}|^2 + \overline{z'-z}|^2\right)}}$. The expression then which the attraction of the whole adds to U is the sum of all the quantities $\frac{\delta M}{\sqrt{\left(\overline{x'-x}|^2 + \overline{y'-y}|^2 + \overline{z'-z}|^2\right)}}$, or the sum of the products of each particle by the reciprocal of its distance from the attracted point; that is, V. The whole value of U then is $V + \frac{4\pi}{3} \cdot \frac{3\pi}{2T^2} \cdot r^2 \cdot \overline{1-\mu^2}$; and the equation to a surface of equal density is

$$C = \frac{4\pi}{3}\left\{\frac{\phi(\alpha)}{r} + \frac{3}{2}\{\tau(a) - \tau(\alpha)\} + \left(\frac{\psi(\alpha)}{r^3} + r^2\{\chi(a) - \chi(\alpha)\}\right) \cdot \frac{1-3\mu^2}{5} + \frac{3\pi}{2T^2} r^2 \cdot \overline{1-\mu^2} + \left(\frac{v(\alpha)}{r^5} + \frac{2}{9} r^4 \{\sigma(a) - \sigma(\alpha)\}\right) \cdot \left(\frac{9}{70} - \frac{9}{7}\mu^2 + \frac{3}{2}\mu^4\right)\right\}$$

(14.) If then for r we put its value in terms of the semi-axis of the spheroid which terminates it, and the angle which it makes with the equator, we shall have an equation which, so far as it depends on that angle, must be identically true.

Now if ε and E are the same functions of α which e and A are of a, $\frac{1}{r}$ to the second order $= \frac{1}{\alpha}\left(1 - \varepsilon \,.\, \overline{1-\mu^2} + \varepsilon^2 \,.\, \overline{1 - \frac{\mu^2}{2} - \frac{\mu^4}{2}} + \mathrm{E} \,.\, \overline{\mu^2 - \mu^4}\right)$: $\frac{1}{r^3}$ to the first order $= \frac{1}{\alpha^3}\left(1 - 3\varepsilon \,.\, \overline{1-\mu^2}\right)$; $r^2 = \alpha^2\left(1 + 2\varepsilon \,.\, \overline{1-\mu^2}\right)$; $\frac{1}{r^5}$ including no small quantities $= \frac{1}{\alpha^5}$; $r^4 = \alpha^4$. These are not to be taken farther, because $\psi(\alpha)$, $\chi(\alpha)$, and $\frac{3\pi}{2\mathrm{T}^2}$, are of the first order, and $\upsilon(\alpha)$ and $\sigma(\alpha)$ of the second order. Substituting these values in the last equation, we find $\mathrm{C} =$

$$\frac{4\pi}{3}\left\{\begin{array}{l} \frac{\varphi(\alpha)}{\alpha} + \frac{3}{2}\{\tau(\mathrm{a}) - \tau(\alpha)\} \\ - \frac{\varepsilon\varphi(\alpha)}{\alpha} \,.\, \overline{1-\mu^2} + \left(\frac{\psi(\alpha)}{\alpha^3} + \alpha^2\{\chi(\mathrm{a}) - \chi(\alpha)\}\right) \,.\, \frac{1-3\mu^2}{5} + \frac{3\pi}{2\mathrm{T}^2}\alpha^2 \,.\, \overline{1-\mu^2} \\ + \frac{\varepsilon^2 . \varphi(\alpha)}{\alpha} \,.\, \overline{1 - \frac{\mu^2}{2} - \frac{\mu^4}{2}} + \frac{\mathrm{E}\varphi(\alpha)}{\alpha}\overline{\mu^2 - \mu^4} - \frac{3\varepsilon\psi(\alpha)}{\alpha^3} \,.\, \overline{1-\mu^2} \,.\, \frac{1-3\mu^2}{5} + \frac{3\pi}{2\mathrm{T}^2} 2\varepsilon\alpha^2 . \overline{1-\mu^2}^2 \\ + 2\varepsilon\alpha^2\{\chi(\mathrm{a}) - \chi(\alpha)\}\,\overline{1-\mu^2} . \frac{1-3\mu^2}{5} + \left(\frac{\upsilon(\alpha)}{\alpha^5} + \frac{2}{9}\alpha^4\{\sigma(\mathrm{a}) - \sigma(\alpha)\}\right) \,.\, \left(\frac{9}{70} - \frac{9}{7}\mu^2 + \frac{3}{2}\mu^4\right) \end{array}\right.$$

Making the coefficient of $\mu^2 = 0$, and that of $\mu^4 = 0$, and observing that, as the equations which we shall find are general for all values of α, we may put a for α,

$$0 = \left\{\begin{array}{l} \frac{e\varphi(a)}{a} - \frac{3}{5} \,.\, \frac{\psi(a)}{a^3} - \frac{3}{5}a^2\{\chi(\mathrm{a}) - \chi(a)\} - \frac{3\pi}{2\mathrm{T}^2}a^2 \\ - \frac{e^2\varphi(a)}{2a} + \frac{A . \varphi(a)}{a} + \frac{12}{5} \,.\, \frac{e\psi(a)}{a^3} - \frac{8}{5}e a^2\{\chi(\mathrm{a}) - \chi(a)\} - \frac{3\pi}{2\mathrm{T}^2} 4 e a^2 \\ \qquad - \frac{9}{7}\left(\frac{\upsilon(a)}{a^5} + \frac{2}{9}a^4\{\sigma(\mathrm{a}) - \sigma(a)\}\right) \end{array}\right.$$

$$0 = \left\{\begin{array}{l} - \frac{e^2\varphi(a)}{2a} - \frac{A\varphi(a)}{a} - \frac{9}{5} \,.\, \frac{e\psi(a)}{a^3} + \frac{6}{5}e a^2\{\chi(\mathrm{a}) - \chi(a)\} + \frac{3\pi}{2\mathrm{T}^2} 2 e a^2 \\ \qquad + \frac{3}{2}\left(\frac{\upsilon(a)}{a^5} + \frac{2}{9}a^4\{\sigma(\mathrm{a}) - \sigma(a)\}\right) \end{array}\right.$$

These are the equations of equilibrium; and since by differentiation they may be reduced to two differential equations, from which the two quantities A and e are to be found, their solution is possible, and the equilibrium with the assumed figure is therefore possible.

(15.) At the surface these equations become

$$0=\left(e-\frac{e^2}{2}+A\right)\frac{\phi(a)}{a}-\left(\frac{3}{5}-\frac{12}{5}e\right).\frac{\psi(a)}{a^3}-\frac{3\pi}{T^2}a^2(1+4e)-\frac{9}{7}.\frac{v(a)}{a^5}.$$

$$0=-\left(\frac{e^2}{2}+A\right)\frac{\phi(a)}{a}-\frac{9}{5}e\frac{\psi(a)}{a^3}+\frac{3\pi}{2T^2}2ea^2+\frac{3}{2}.\frac{v(a)}{a^5}.$$

(16). The force on any point in the direction of r is found by differentiating the expression at the end of (13) with respect to r. For since, by (13), the force in the direction of $x=\frac{dU}{dx}$, if we conceive x to coincide with r, the force in the direction of $r=\frac{dU}{dr}$. In this expression a is a function of r; but it will be found (as may also be proved by independent reasoning) that the terms produced by differentiation with respect to a destroy each other. The force then in the direction of r (with its sign changed, as we have to estimate not a repulsive but an attractive force) is

$$\frac{4\pi}{3}\left\{\frac{\phi(a)}{r^2}-\left(-\frac{3\psi(a)}{r^4}+2r\{\chi(\mathrm{a})-\chi(a)\}\right).\frac{1-3\mu^2}{5}-\frac{3\pi}{2T^2}.2r.\overline{1-\mu^2}\right.$$
$$\left.-\left(-\frac{5v(a)}{r^6}+\frac{8}{9}r^3\{\sigma(\mathrm{a})-\sigma(a)\}\right).\left(\frac{9}{70}-\frac{9}{7}\mu^2+\frac{3}{2}\mu^4\right)\right\}.$$

At the external surface this is $\frac{4\pi}{3}\left\{\frac{\phi(a)}{r^2}+\frac{3}{5}.\frac{\psi(a)}{r^4}.\overline{1-3\mu^2}-\frac{3\pi}{2T^2}.2r.\overline{1-\mu^2}+\frac{v(a)}{r^6}\left(\frac{9}{14}-\frac{45}{7}\mu^2+\frac{15}{2}\mu^4\right)\right\}$. Observing that $\frac{1}{r^2}=\frac{1}{a^2}\left(1-2e.\overline{1-\mu^2}+3e^2\,\overline{1-\mu^2}+2A.\overline{\mu^2-\mu^4}\right)$; $\frac{1}{r^4}=\frac{1}{a^4}\left(1-4e.\overline{1-\mu^2}\right)$: $r=a(1+e.\overline{1-\mu^2})$: $\frac{1}{r^6}=\frac{1}{a^6}$; the force on a point of the surface in the direction of

$$r=\frac{4\pi}{3}\left\{\frac{\phi(a)}{a^2}-\frac{2e.\phi(a)}{a^2}.\overline{1-\mu^2}+\frac{3}{5}.\frac{\psi(a)}{a^4}\,\overline{1-3\mu^2}-\frac{3\pi}{2T^2}.2a.\overline{1-\mu^2}+\frac{3e^2.\phi(a)}{a^2}.\overline{1-\mu^2}+\frac{A2.\phi(a)}{a^2}.\overline{\mu^2-\mu^4}\right.$$
$$\left.-\frac{12}{5}.\frac{e.\psi(a)}{a^4}.\overline{1-\mu^2}.\overline{1-3\mu^2}-\frac{3\pi}{2T^2}.2ea.\overline{1-\mu^2}^2+\frac{v(a)}{a^6}\left(\frac{9}{14}-\frac{45}{7}\mu^2+\frac{15}{2}\mu^4\right)\right\}.$$

(17.) Now since this is the resolved part of the whole gravity at any point, which is necessarily perpendicular to the surface, we shall find the whole gravity by dividing this expression by the cosine of the angle of the vertical. To the order which we are considering, the angle of the vertical is the same in the spheroid and in the ellipsoid with the same axes, and it is therefore $= 2\,\underline{e}\,.$ sin lat. cos lat. $= 2\,e\,.\,\mu\sqrt{1-\mu^2}$. Its cosine therefore $= 1 - 2\,e^2\,.\,\overline{\mu^2 - \mu^4}$: consequently gravity at any point $= \frac{4\pi}{3}\Big\{\frac{\varphi(a)}{(a)^2} - \frac{2e\,.\,\varphi(a)}{a^2}\,.\,\overline{1-\mu^2} + \frac{3}{5}\,.\,\frac{\psi(a)}{a^4}\,.\,\overline{1-3\mu^2} - \frac{3\pi}{2T^2}\,.\,2a\,.\,\overline{1-\mu^2} + \frac{2e^2\,.\,\varphi(a)}{a^2}\,.\,\overline{\mu^2-\mu^4} + \frac{3e^2\,.\,\varphi(a)}{a^2}\,.\,\overline{1-\mu^2} + \frac{2A\varphi(a)}{a^2}\,.\,\overline{\mu^2-\mu^4} - \frac{12}{5}\,.\,\frac{e\,.\,\psi(a)}{a^4}\,.\,\overline{1-\mu^2\,.\,1-3\mu^2} - \frac{3\pi}{2T^2}\,.\,2ea\,.\,\overline{1-\mu^2}^{\,2} + \frac{v(a)}{a^6}\Big(\frac{9}{14} - \frac{45}{7}\mu^2 + \frac{15}{2}\mu^4\Big)\Big\}$. But this is in terms of μ, the sine of the corrected latitude: it will be more convenient to have it in terms of λ, the sine of the real latitude. Since the corr. lat. = real lat. — $2\,e$. sin. lat. cos. lat. it is easily found that $\mu^2 = \lambda^2 - 4\,e\,.\,\overline{\lambda^2 - \lambda^4}$: substituting, gravity $= \frac{4\pi}{3}\Big\{\frac{\varphi(a)}{a^2}(1 - 2e + 3e^2) + \frac{\psi(a)}{a^4}\Big(\frac{3}{5} - \frac{12}{5}e\Big) - \frac{3\pi}{2T^2}\,a\,(2 + 2e) + \frac{9}{14}\,.\,\frac{v(a)}{a^6} + \Big(\frac{\varphi(a)}{a^2}(2e - 3e^2) - \frac{\psi(a)}{a^4}\Big(\frac{9}{5} - \frac{12}{5}e\Big) + \frac{3\pi}{2T^2}\,a\,(2 + 2e) + \frac{15}{4}\,.\,\frac{v(a)}{a^6}\Big)\lambda^2 - \Big(\frac{\varphi(a)}{a^2}(6e^2 - 2A) - \frac{72}{5}\,.\,\frac{e\,.\,\psi(a)}{a^4} + \frac{3\pi}{2T^2}\,6ae + \frac{15}{2}\,.\,\frac{v(a)}{a^6}\Big)\,.\,\lambda^2\,.\,\overline{1-\lambda^2}\Big\}$.

(18.) From the equations of (15), $\frac{\psi(a)}{a^4} = \frac{\varphi(a)}{a^2}\Big(\frac{5e}{3} + \frac{5e^2}{6} + \frac{5A}{21}\Big) - \frac{3\pi}{2T^2}\,a\,\Big(\frac{5}{3} + \frac{130}{21}e\Big)$; $\frac{v(a)}{a^6} = \frac{\varphi(a)}{a^2}\Big(\frac{7e^2}{3} + \frac{2A}{3}\Big) - \frac{3\pi}{2T^2}\,.\,\frac{10}{3}\,ae$. Substituting these, gravity

$$= \frac{4\pi}{3}\Big\{\frac{\varphi(a)}{a^2}\Big(1 - e + e^2 + \frac{4A}{7}\Big) - \frac{3\pi}{2T^2}\,a\,\Big(3 + \frac{27}{7}e\Big)$$

$$+ \Big(\frac{\varphi(a)}{a^2}\Big(-e + 2e^2 + \frac{2}{7}A\Big) + \frac{3\pi}{2T^2}\,a\,\Big(5 + \frac{39}{7}e\Big)\Big)\,.\,\lambda^2$$

$$- \Big(\frac{\varphi(a)}{a^2}\Big(-\frac{e^2}{2} + 3A\Big) + \frac{3\pi}{2T^2}\,5ae\Big)\,.\,\lambda^2\,.\,\overline{1-\lambda^2}\Big\}\,.$$

(19.) Let m be the ratio of the centrifugal force at the equator to gravity at the equator. The centrifugal force there $= \frac{4\pi^2}{T^2} a (1 + e) = \frac{4\pi}{3} . \frac{3\pi}{2T^2} 2a(1 + e)$: the gravity there $= \frac{4\pi}{3} \left\{ \frac{\varphi(a)}{a^2} (1 - e) - \frac{3\pi}{2T^2} . 3a \right\}$: therefore $\frac{3\pi}{2T^2} a (2 + 2e) = \frac{\varphi(a)}{a^2} (m - me) - \frac{3\pi}{2T^2} a . 3m$; whence $\frac{3\pi}{2T^2} a = \frac{\varphi(a)}{a^2} . \frac{m - me}{2 + 2e + 3m} = \frac{\varphi(a)}{a^2} \left(\frac{m}{2} - me - \frac{3m^2}{4} \right)$. Making this substitution, gravity $= \frac{4\pi}{3} . \frac{\varphi(a)}{a^2} \left\{ (1 - e - \frac{3}{2} m + e^2 + \frac{15}{14} me + \frac{9}{4} m^2 + \frac{4}{7} A) + \left(\frac{5m}{2} - e + 2e^2 - \frac{31}{14} me - \frac{15}{4} m^2 + \frac{2}{7} A \right) \lambda^2 - \left(\frac{5}{2} me - \frac{e^2}{2} + 3A \right) . \lambda^2 . \overline{1 - \lambda^2} \right\}$ $= \frac{4\pi}{3} . \frac{\varphi(a)}{a^2} \left(1 - e - \frac{3m}{2} + e^2 + \frac{15}{14} me + \frac{9}{4} m^2 + \frac{4}{7} A \right)$ $\times \left\{ 1 + \left(\frac{5m}{2} - e + e^2 - \frac{17}{14} me + \frac{2}{7} A \right) . \lambda^2 - \left(\frac{5}{2} me - \frac{e^2}{2} + 3A \right) . \lambda^2 . \overline{1 - \lambda^2} \right\}$. If then the equatorial gravity be represented by G, the gravity in latitude l will be

$$G \left\{ 1 + \left(\frac{5m}{2} - e + e^2 - \frac{17}{14} me + \frac{2}{7} A \right) \sin.^2 l \right.$$
$$\left. - \left(\frac{5}{2} me - \frac{e^2}{2} + 3A \right) \sin^2 l . \cos^2 l \right\}$$

Whatever therefore be the law of density in the interior of the earth, the gravity at any point of the surface can be expressed from a knowledge of the form of that surface only. This is an extension of CLAIRAUT's theorem.

(20). We shall now proceed to find an expression for the length of an arc of the meridian included between two given latitudes. If θ be the corrected latitude, and $u = \frac{1}{r}$, the radius of curvature is $\frac{\left\{ 1 + \frac{1}{u^2} . \left(\frac{du}{d\theta} \right)^2 \right\}^{\frac{3}{2}}}{u + \frac{d^2 u}{d\theta^2}}$. Here $u = \frac{1}{a} (1 - e . \overline{1 - \mu^2} + e^2 . \overline{1 - \frac{\mu^2}{2} - \frac{\mu^4}{2}} + A . \overline{\mu^2 - \mu^4}) = \frac{1}{a} (1 - \frac{e}{a} + \frac{9e^2}{16} + \frac{A}{8} -$

$\left(\frac{e}{2}-\frac{e^2}{2}\right)\cos 2\theta-\left(\frac{e^2}{16}+\frac{A}{8}\right)\cos 4\theta\Big)$, and the radius of curvature $= a\left\{1+\frac{e}{2}+\frac{25e^2}{16}-\frac{A}{8}-\frac{3e}{2}\cos 2\theta-\left(\frac{9e^2}{16}+\frac{15}{8}A\right)\cos 4\theta\right\}$. Let this $=\gamma$. It was found that $\mu^2=\lambda^2-4e.\overline{\lambda^2-\lambda^4}$, or $\sin^2\theta=\sin^2 l-4e.\sin^2 l.\cos^2 l$; from which $\cos 2\theta = \cos 2l+e-e\cos 4l$: hence $\gamma=a\left\{1+\frac{e}{2}+\frac{e^2}{16}-\frac{A}{8}-\frac{3e}{2}\cos 2l+\left(\frac{15e^2}{16}-\frac{15}{8}A\right)\cos 4l\right\}$. Now since l is the angle made by the normal with a fixed line, the increment of the arc, corresponding to the increment δl of the latitude, is ultimately $\gamma\,\delta l$, and the arc $=\int_l\gamma$; integrating therefore from $l=L$ to $l=L'$, and making $a\left(1+\frac{e}{2}+\frac{e^2}{16}-\frac{A}{8}\right)=R$, the arc included between the latitudes L and $L'=$

$$R\left\{L'-L-\left(\frac{3e}{4}-\frac{3e^2}{8}\right).(\sin 2L'-\sin 2L)\right.$$
$$\left.+\left(\frac{15e^2}{64}-\frac{15A}{32}\right).(\sin 4L'-\sin 4L)\right\}.$$

(21.) An arc of a parallel in latitude l, comprehending the difference of longitude D, is most easily obtained by observing that the decrement of its radius upon increasing l is equal to $\sin l\times$ the increment of the arc $=\sin l\times\gamma\,\delta l$, and therefore the radius $=-\int_l\gamma\sin l$; the integral being corrected so as to become 0 when $l=90°$. We must therefore integrate $-R\sin l\left\{1-\left(\frac{3e}{2}-\frac{3e^2}{4}\right)\cos 2l+\left(\frac{15e^2}{16}-\frac{15A}{8}\right)\cos 4l\right\}$. This gives the radius of the parallel $=R\left\{\left(1+\frac{3e}{4}-\frac{3e^2}{8}\right)\cos l-\left(\frac{e}{4}+\frac{e^2}{32}-\frac{5A}{16}\right)\cos 3l+\left(\frac{3e^2}{32}-\frac{3A}{16}\right)\cos 5l\right\}$; and therefore the arc corresponding to the difference of longitude

$$D = D.R.\left\{\left(1+\frac{3e}{4}-\frac{3e^2}{8}\right)\cos l-\left(\frac{e}{4}+\frac{e^2}{32}-\frac{5A}{16}\right)\cos 3\,l\right.$$
$$\left.+\left(\frac{3}{32}e^2-\frac{3A}{16}\right)\cos 5\,l\right\}.$$

I now proceed to compare this theory with observation. In selecting from the numerous determinations of the variation of gravity one set for our present purpose, there can be no hesitation in fixing on Captain SABINE's, as extending over the greatest arc of latitude, and as being made by the same observer with the same instruments at no great interval of time. The following are the places of his observations and their latitudes, with the lengths of the seconds' pendulum determined by him.

Place.	Latitude.			Length of seconds' pendulum in inches.
	°	′	″	
St. Thomas .		24	41,2	39,02074
Maranham .	2	31	43,3	,01214
Ascension	7	55	47,8	,02410
Sierra Leone .	8	29	27,9	,01997
Trinidad . .	10	38	56	,01884
Bahia . . .	12	59	21	,02425
Jamaica . .	17	56	7,6	,03510
New York .	40	42	43,2	,10168
London . .	51	31	8,4	,13929
Drontheim .	63	25	54,2	,17456
Hammerfest .	70	40	5,3	,19519
Greenland .	74	32	18,6	,20335
Spitzbergen .	79	49	57,8	,21469

The irregularities in the lengths at the places nearest to the equator are considerable; but the number of these places is so great, that the errors will probably destroy each other. Assuming the length of the seconds' pendulum $= M + N \cos 2 \text{ lat} + P \cos 4 \text{ lat}$. the errors are

$$- 39{,}02074 + M + N \times {,}99989 + P \times {,}99959$$
$$- 39{,}01214 + M + N \times {,}99610 + P \times {,}98445$$
$$- 39{,}02410 + M + N \times {,}96194 + P \times {,}85063$$
$$- 39{,}01997 + M + N \times {,}95640 + P \times {,}82938$$
$$- 39{,}01884 + M + N \times {,}93171 + P \times {,}73611$$
$$- 39{,}02425 + M + N \times {,}89895 + P \times {,}61626$$
$$- 39{,}03510 + M + N \times {,}81034 + P \times {,}31330$$
$$- 39{,}10168 + M + N \times {,}14910 - P \times {,}95552$$
$$- 39{,}13929 + M - N \times {,}22559 - P \times {,}89821$$
$$- 39{,}17456 + M - N \times {,}59992 - P \times {,}28024$$
$$- 39{,}19519 + M - N \times {,}78082 + P \times {,}21936$$
$$- 39{,}20335 + M - N \times {,}85784 + P \times {,}47184$$
$$- 39{,}21469 + M - N \times {,}93767 + P \times {,}75849$$

To determine M, N, and P, by the method of least squares, we must form three equations, by making equal to nothing the sum of these errors—first, when each is multiplied by the coefficient of M in that line, then when each is multiplied by the coefficient of N, and finally when each is multiplied by the coefficient of P. Thus we find

$$0 = - 508{,}18390 + M \times 13 + N \times 3{,}30259 + P \times 4{,}64544$$
$$0 = - 128{,}29476 + M \times 3{,}30259 + N \times 8{,}82265 + P \times 4{,}02629$$
$$0 = - 181{,}31213 + M \times 4{,}64544 + N \times 4{,}02629 + P \times 7{,}04391$$

By the solution of these equations $M = 39{,}11647$: $N = - {,}10146$; $P = {,}00106$; and the length of the seconds' pendulum in inches $= 39{,}11647 - {,}10146 \times \cos 2 \text{ lat} + {,}00106$

$\times \cos 4 \text{ lat} = 39{,}01606 + {,}20292 \times \sin^2 \text{lat} - {,}00844 \times \sin^2 \text{lat} . \cos^2 \text{lat} . = 39{,}01606 \left\{ 1 + {,}005201 \times \sin^2 \text{lat} - {,}000216 \times \sin^2 \text{lat} . \cos^2 \text{lat} \right\}$; and gravity is proportional to this. Comparing the coefficients of $\sin^2$ lat and $\sin^2$ lat . $\cos^2$ lat with those in the expression of Art. 19, we have the following equations

$$\frac{5m}{2} - e + e^2 - \frac{17}{14} m e + \frac{2}{7} A = {,}005201$$

$$\frac{5}{2} m e - \frac{e^2}{2} + 3 A \qquad = {,}000216$$

From the length of the equatorial pendulum, supposing the equatorial radius of the earth = 3486908 fathoms, we find $m = {,}003464$. And in the terms of the second order instead of e we may put $\frac{1}{290}$, which is certainly not far from the truth. Making these substitutions, the equations become

$${,}008657 - e + \frac{2}{7} A = {,}005201$$

$${,}000024 + 3 A \qquad = {,}000216$$

From these, $e = {,}003474 = \frac{1}{287{,}8}$; and $A = 000064$. The sign of A shows that the earth is less protuberant at the latitude of 45° than an ellipsoid of the same polar and equatorial radii. For the elevation of the spheroid above the ellipsoid $= a A . \mu^2 . \overline{\mu^2 - 1}$, where μ = sine of latitude; making the latitude = 45°, $\mu = \frac{1}{\sqrt{2}}$, and the elevation of the spheroid is $-\frac{A}{4}$: as A here is positive, this elevation is negative, or the spheroid at that latitude is depressed below the ellipsoid. This I should be inclined to expect: for though I have not been able to solve the differential equation in *A*, even in the cases in which the differential equation for *e* can be solved to the first order, yet by the examination of a simple hypothesis (that of a homogeneous fluid whose density is very small surrounding a central nucleus), it may easily be seen that in this case the spheroid is not so far protuberant in middle latitudes as the ellipsoid. If the attraction of the fluid be neglected, and the central mass be called m, the force on any point in the direction of $x = \frac{-mx}{(x^2+y^2+z^2)^{\frac{3}{2}}}$: that in the direction of $y = \frac{-my}{(x^2+y^2+z^2)^{\frac{3}{2}}} + \frac{4\pi^2}{T^2}y$; that in $z = \frac{-mz}{(x^2+y^2+z^2)^{\frac{3}{2}}} + \frac{4\pi^2}{T^2}z$. Hence $U = \frac{m}{\sqrt{(x^2+y^2+z^2)}} + \frac{2\pi^2}{T^2}\overline{y^2+z^2} = C$; or if $y^2+z^2 = v^2$, $\frac{m}{\sqrt{x^2+v^2}} + \frac{2\pi^2}{T^2}v^2 = C$; whence $x^2 = \frac{m^2}{\left(c - \frac{2\pi^2}{T^2}v^2\right)^2} - v^2 = \frac{m^2}{C^2} - \left(1 - \frac{4\pi^2 m^2}{C^3 T^2}\right)v^2 + \frac{6\pi^4 m^2}{C^4 T^4}v^4$ nearly, supposing $\frac{2\pi^2}{CT^2}$ small; and this may be put under the form $(a^2 - v^2)(n - pv^2)$, from which the proposition is evident. I conclude therefore that the results of Captain SABINE's observations, as far as they go, are in strict agreement with theory.

I now come to the comparison of the expression for the length of a meridian arc with those arcs which have been most accurately measured. The influence of local circumstances on these measures, it is well known, is greater than that on pendulum experiments; and the discordances in different measures are such that, in order to get a result of any exactness, we must confine ourselves to the longest arcs which have been measured with the greatest care. I have selected the following: 1. BOUGUER's arc in Peru: 2. LAMBTON's whole arc in India: 3. The French arc from Formentera to

Dunkirk: 4. The English arc from Dunnose to Clifton: 5. The Swedish arc from Mallorn to Pahtavara.

The Peruvian arc I have taken as recalculated by DELAMBRE in the Base du Systeme Metrique, Tome iii. p. 112, &c. I have supposed the toise of Peru equal to 1,065766 fathoms.

The Indian arc is taken as finally corrected by Colonel LAMBTON in the Philosophical Transactions for 1823.

In the estimation of the length of a part of the French arc, which Colonel LAMBTON has given in the Philosophical Transactions for 1818, and which Captain KATER has sanctioned by copying without any remark, there appears to be a serious error, arising from an unnecessary reduction for temperature. As I am sensible that I am now opposed to two Gentlemen whose assertion on such a point is almost decisive, I will state distinctly the reasons which have led me to this conclusion.

In the measure of the base at Melun (Base du Systeme Metrique, Tome ii. p. 44), the length of the base was reduced to the length which it would have had if the measuring rods had been used at the temperature of 13° of REAUMUR, or 16°¼ centigrade. The number of rods employed was four; but the rod or module No. 1 was compared with each of the others, and the base was expressed by the length of the module No. 1 at the temperature of 16°¼ centigrade.

From the logarithm set down in p. 698, it appears that this was the length used in the calculation of the triangles.

The module No. 1 was found at this temperature to be less than two toises (the toise being the length of an iron standard, called the toise of Peru, at the temperature of 13° of REAUMUR); but from an error in the zero point of the metallic thermometers there were some errors in the reduction of the base, which exactly corrected the difference (Tome iii. p. 136). The semi-module, therefore, in the expression for the arc of the meridian, is exactly equal to the toise of Peru at the temperature of 16°$\frac{1}{4}$ centigrade.

The standard mètre at the temperature of 0° centigrade = 443,296 lines of the toise of Peru at 16°$\frac{1}{4}$ centigrade (p. 139).

The standard mètre at the temperature of 0° centigrade was found by Captain KATER (Philosophical Transactions 1818) to be = 39,37079 inches of the English standard at the temperature of 62° Fahrenheit.

Consequently the semi-module in the expression for the arc of the meridian is = 1,065766 fathoms measured by the English standard at the temperature of 62° FAHRENHEIT; and since the English and Indian measures are estimated by this standard at the same temperature, no reduction is to be made for temperature. (The degree between Paris and Evaux is found thus to be 60822,5 fathoms; Col. LAMBTON has stated it to be 60779 fathoms). The length of the French arc given below, in fathoms, is therefore found by merely multiplying the number of semi-modules by 1,065766.

The English arc has the corrections which Captain KATER has given in the Phil. Trans. for 1821.

The Swedish arc is taken from SVANBERG's Exposition des Operations, &c. supposing their standard compared with the French standard at the temperature of 0° centigrade.

Thus the following table is formed.

Place.	L	L′	Length in Fathoms.
	° ′ ″	° ′ ″	
Peru . .	−0 2 31,22	3 4 31,9	188510
India . .	8 9 38,39	18 3 23,6	598630
France . .	38 39 56,11	51 2 9,2	751567
England .	50 37 5,27	53 27 29,89	172751
Sweden .	65 31 30,27	67 8 49,55	98870

In the application of the method of least squares, it must be observed, that the accuracy of the terrestrial measures can scarcely be questioned, and that the chance of errors in the determination of the extreme latitudes, arising either from faults of observation or from local attractions, is principally to be considered. This however amounts to the same as supposing an error in the length of the meridian arc. Assuming the form, $M \times$ number of seconds in $\overline{L'-L} + N(\sin 2L' - \sin 2L) + P(\sin 4L' - \sin 4L)$, the errors in the lengths are

$$\begin{aligned}
&-188510 + M \times 11223{,}1 + N \times {,}1086 + P \times {,}216\\
&-598630 + M \times 35625{,}2 + N \times {,}3084 + P \times {,}412\\
&-751567 + M \times 44533{,}1 + N \times {,}0023 - P \times {,}837\\
&-172751 + M \times 10224{,}6 - N \times {,}0241 - P \times {,}175\\
&-\ \ 98870 + M \times \ \ 5839{,}3 - N \times {,}0383 - P \times {,}009
\end{aligned}$$

and the equations formed in the same manner as before are

$$\begin{aligned}
0 &= -59255232 + M \times 3516949{,}8 + N \times 11{,}8382 - P \times 22{,}016\\
0 &= -198869 + M \times 11838{,}2 + N \times {,}1090 + P \times {,}153\\
0 &= \ \ 37285 - M \times 2201{,}6 + N \times {,}0153 + P \times {,}093
\end{aligned}$$

from which $M = 16{,}88164$; $N = -9358$; $P = 267$; and the length of an arc of the meridian in fathoms =

$16{,}88164 \times$ n°. of seconds in $\overline{L' - L} - 9358 \times (\sin 2L' - \sin 2L) + 267 \times (\sin 4L' - \sin 4L)$.

The lengths of the several arcs found from this expression differ from those above by -4, $+4$, -19, $+35$, and $+63$ fathoms respectively. The largest of these errors falls (as will generally be the case when the method of least squares is used) on that length in which the coefficients of M, N, and P, are smallest; but in a mountainous country, I conceive that an error of less than $4''$ in the difference of latitudes is by no means inadmissible. And I am the more inclined to allow this error, because, upon applying the formula to the arc measured in Sweden by the French Academicians, the error, which is much greater than this, has a different sign.

I may here perhaps without impropriety, make some remarks on the credit which appears to be due to the French measure in Sweden. That measure has often been mentioned with contempt; but more particularly since the late measure by Svanberg. I have not however been able to discover any reason for this contempt, except the disagreement of its results from those of other operations. The last part of the process, it is well known, was the measure of the base; and the Academicians themselves were so much astonished at the result, that they immediately proceeded to verify every part of the measure, particularly the latitudes. And the general accuracy of the measure has been confirmed by the late measure. The triangles were admirably chosen; the length of the arc agrees within a few fathoms with that found by Svanberg; the latitude of the southern extremity at Tornea is precisely the same as that calculated by Svanberg from the observations at Mallorn; the latitude of Kittis, their northern station, unfortunately was not examined. The presumption, I think is, that though a part of the discrepancy may be attributed to errors of observation, yet a great part of it must be due to the irregularities of local attraction in so rugged a country (though Svanberg appears to think this impossible), and that the new measure probably is not free from errors of the same kind. The measure at the Cape of Good Hope, conducted by the ablest astronomer of the age, has generally been thought inadmissible for the same reason.

Since $L' - L = \sin 1'' \times$ number of seconds in $L' - L$, we have for the length of a meridian arc $\frac{16{,}88164}{\sin 1''} \left\{ L' - L - \frac{9358 \times \sin 1''}{16{,}88164} \times (\sin 2L' - \sin 2L) + \frac{267 \times \sin 1''}{16{,}88164} (\sin 4L' - \sin 4L) \right\}$. Comparing this with the formula in (20), $\frac{3e}{4} - \frac{3e^2}{8} = \frac{9358 \times \sin 1''}{16{,}88164}$, and $\frac{15e^2}{64} - \frac{15A}{32} = \frac{267 \times \sin 1''}{16{,}88164}$. From these equations $e = {,}003589 = \frac{1}{278{,}6}$; $A = -{,}000157$. The difference between the polar and equatorial axes is even greater than that assigned by Captain Sabine. But the most striking difference in the deductions is that the value of *A*, now found, has a negative sign; which would seem to indicate that the earth is protuberant at the latitude 45° above the ellipsoid, which has the same axes. And it does not appear that by any alteration of the values of A and e it is possible to reconcile the different observations. If we suppose the Indian and French arcs to be quite accurate, we shall find $e = {,}003269 - A \times 2{,}139$: this evidently cannot be reconciled with the values of e and A deduced from the pendulum experiments.

On the whole I conceive, that we cannot assert that, on the figure of the earth, theory and observation are in perfect agreement. If the arcs which have been measured were no longer than that in Sweden, or even than that in England, I should not hesitate in attributing to errors of observation and local attraction a considerable part of the discrepancy. And had the theory been confined to terms of the first order, I should have thought it probable that terms of importance might be omitted in the second and higher orders. But it appears that with the improved theory, applied to the comparison of the Indian and French arcs, the shorter of which has an amplitude of nearly ten degrees, it is impossible to establish any agreement with the pendulum observations of Captain SABINE; and I know no other series which for extent and other advantages can be compared with those.

The measures of arcs of the meridian which have hitherto been made are, I imagine, insufficient for the determination of the figure of the earth. The arcs in India and France are the only ones in which the possible errors in latitude would not bear a sensible proportion to the effects of ellipticity. Captain SABINE has proposed to measure an arc in Spitzbergen. The shortness of the arc, and the mountainous character of the country, would make it almost useless. The desideratum at present is an extensive arc in a high latitude. We have two good arcs near the equator; of which the Indian, at least, is as long as can be desired. We have a still longer arc almost exactly bisected by the parallel of 45°. With an arc of equal length in the neighbourhood of the pole, we might determine the three terms in the expression for an arc of the meridian to great accuracy. This perhaps it will be impossible ever to execute.

There are some measures in which such extraordinary disturbances appear to have existed, that it seems desirable to examine and extend them: of these, the most remarkable is that made by **Lacaille** at the Cape of Good Hope. And perhaps in speaking of the probable effects of local attraction, I may be permitted to allude to one of a different kind in England. This is the correction which it has been found necessary to apply to the longitudes of places in England, as deduced from the observations made at Beachy Head and Dunnose. For this investigation I prefer the method of **Dalby**, explained in the Philosophical Transactions for 1790 and 1795, to any other; as being unobjectionable on the ground of accuracy, and as applying to any surface in which the intersections of the normals at the two stations with the earth's axis, or with a line parallel to the earth's axis, are not very distant. The difference of longitude is then made to depend on this case of spherical trigonometry: given two sides (a, b) and the sum of the opposite angles $(A + \mathrm{B})$, to find the third angle (c). The formula is, $\tan. \frac{c}{2} = \frac{\cos. \frac{a-b}{2}}{\cos. \frac{a+b}{2}} \cos. \frac{A+\mathrm{B}}{2}$.

In this instance $a =$ colatitude of Dunnose $= 39^\circ\ 22'\ 52'',69$; $b =$ colatitude of Beachy Head $= 39^\circ\ 15'\ 36'',29$; $A + \mathrm{B} =$ sum of observed azimuths $= 178^\circ\ 52'\ 51''$; hence $\mathrm{C} = 1^\circ\ 26'\ 47'',87$. And if the errors in the observed quantities a, b, A, B, were $\delta a, \delta b, \delta A, \delta \mathrm{B}$, the error in C would be

$$-\frac{\cos. \frac{a-b}{2} . \cos.^2 \frac{\mathrm{C}}{2}}{\cos. \frac{a+b}{2} . \sin.^2 \frac{A+\mathrm{B}}{2}} (\delta A + \delta \mathrm{B}) + \frac{\cos. \frac{A+\mathrm{B}}{2} . \cos.^2 \frac{\mathrm{C}}{2}}{\cos.^2 \frac{a+b}{2}} (\sin. b. \delta a + \sin. a. \delta b) = -1,293 (\delta A + \delta \mathrm{B}) + ,0103 . \delta a + ,0104 . \delta b.$$

Now the chronometer observations for the difference of longitude of Dover and Falmouth, according to Dr. Tiarks, (Phil. Trans. 1824) give by proportion the difference of longitude of Dunnose and Beachy Head 1° 27′ 4″,75. The discrepance then is nearly 17″; which (since a small error in *a* or *b* produces an almost insensible effect on C) implies an error of 13″ in the sum of the azimuths. This I should think is absolutely impossible. We must suppose then either that, in consequence of some local attraction, the apparent difference of longitudes was altered, or that some error has crept into the observations of Dr. Tiarks. The method of obtaining apparent time by corresponding altitudes, which was employed by Dr. Tiarks, is not the most accurate, nor perhaps the most proper, when the whole time in dispute is only five seconds. As Dr. Tiarks has not given the details of the observations, it is impossible to form any correct estimation of their accuracy. If however any local attraction have drawn aside the plummet at the eastern station towards the east, and that at the western station towards the west, the longitude would thus be made to appear smaller than it really is. The error produced in the longitude of either place would be $= \frac{\text{deviation}}{\text{cos. lat.}}$: and thus an error in longitude of 17″ would be accounted for by supposing the plummet at Beachy Head drawn 5″,4 to the east, and that at Dunnose 5″,4 to the west. Each of these is a larger deviation than we have reason to believe has taken place at any station in England; and it appears therefore desirable that the observations should be re-examined. This operation would not be difficult: the easiest method perhaps would be by rocket signals; and it is obvious, that the local causes which affected the difference of longitude found by observations of azimuth, would produce exactly the same effect by disturbing the levels of the transit or other instruments used to obtain the time. The immediate effect of such observations then, would be merely to confirm or to refute the conclusions deduced from the observations of azimuth: if the observations of Dr. Tiarks should be considered free from objection, they would serve to establish, or to destroy the belief, that local attraction may produce sensible disturbances in longitude.

G. B. AIRY.

Since the above was written, I have been favoured by Captain Sabine with a more detailed statement than I had before seen of the extent and probable circumstances of an arc in Spitzbergen. It appears that an arc might be measured extending nearly from latitude 76° 25′ to 80° 35′: the extremities of which would be on islands at a small distance from the main land of Spitzbergen. With the same formula as before, the error to be used in the method of least squares would be

$$-\text{measured length} + M \times 15000 - N \times ,1338 + P \times ,201.$$

Upon comparing this expression with those given before, it will be observed, that this is the only error in which the coefficient of N has a large negative value; and that from the preponderance at present of negative signs in the coefficients of P, an error with a positive coefficient of P would be very desirable. On these accounts then, an arc of that extent in that latitude would contribute much to our knowledge of the figure of the earth. But it is very likely that there would be sensible disturbances in the latitudes of the extreme stations. We cannot imagine a situation in which there is a greater probability that the difference of latitudes would be made too large, than when the latitudes are observed at two stations at the level of the sea, with high land between them. But this would depend much on the nature of the rocks. In Peru, for instance, though far more mountainous, it is probable that the disturbance was not great. If then, as Captain SABINE proposes, any preliminary survey be made, it would perhaps be proper to examine not only the circumstances which would affect the practical facility of the operation, but also those which might have an influence on the determination of the latitudes; and should these be found not very unfavourable, a meridian arc of four degrees measured with great care would be highly valuable.

ADDITIONS.

THE equations of Art. 15, it will easily be seen, are equally true, whether the interior of the mass be supposed to be fluid, or to consist of solid shells of different density, in which the radius of each separating surface is expressed by the formula $a \left\{ 1 + e . \overline{1 - \mu'^2} - \left(\frac{3e^2}{2} + A \right) \overline{. \mu'^2 - \mu'^4} \right\}$. And the expression for gravity in Art. 19, and all its consequences, hold equally on either supposition.

A theorem similar to CLAIRAUT's may be shown to be true, to whatever order the investigations be extended. For upon using the value of R found in the investigation above, and upon carrying all the operations one step farther, it would be found that the equation of equilibrium could not be satisfied, in consequence of the introduction of terms multiplying μ'^6, which would not destroy each other. It would be necessary then to assume for R a value of the form

$$a\left\{1+e.\overline{1-\mu'^2}-\left(\frac{3e^2}{2}+A\right).\overline{\mu'^2-\mu'^4}-\left(\frac{e^3}{2}+B\right).\overline{\mu'^2-6\mu'^4+5\mu'^6}\right\}:$$

by which there would be introduced two new functions of *a*, *e*, &c. in the general expression for V, and in that for the force at any point; and one new function of a, e, &c. in the expression for the force at the surface. But as a power of μ' is introduced, which did not appear before, there would be found, by the process of Art. 14, one equation which was not found in the preceding approximation. By this equation the new function could be eliminated, and therefore the force at any point could be expressed in the same manner as in the preceding approximation, namely, by means of one function of (a) multiplying a quantity depending solely on the form of the external surface, and the latitude of the point on that surface. The same may be proved for every succeeding approximation; and thus we arrive at the following theorem: "If a heterogeneous fluid, the particles of which attract each other with accelerating forces inversely proportional to the squares of their distances, revolve round an axis; and if the proportion of the centrifugal force at the equator to the whole force there be given; the force at any point of the surface can be exactly expressed from a knowledge of the form of the surface and the position of that point, without any knowledge of the law of the internal density." This is likewise true if the interior be solid.

G. B. A.

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METEOROLOGICAL JOURNAL

for January, 1825.

1825 January.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♄	1	8	30	30,079	52	795	49		W	2	Cloudy.
		3	0	30,121	54	765	54		W	1	Cloudy.
☉	2	9	0	29,871	45	824	45		W	1	Fine.
		3	0	29,953	44	964	47		NW	1	Fine.
☾	3	9	0	30,345	38	579	37		W	1	Fine.
		3	0	30,203	45	753	45		W	1	Cloudy.
♂	4	9	0	29,907	51	821	44	0.154	W by N	1	Cloudy.
		3	0	30,062	46	932	46		NW	2	Rain.
☿	5	9	0	30,588	36	685	35	0.020	N by W	1	Cloudy and dark.
		3	30	30,651	38	712	38		N	1	Cloudy.
♃	6	9	0	30,716	32½	843	32		N	1	Fine.
		3	0	30,681	38	712	38		W	1	Fine.
♀	7	9	0	30,521	40	943	35		W by N	1	Cloudy.
		3	0	30,453	46	966	46		W	1	Rain.
♄	8	9	0	30,714	38	879	37½	0.002	N	1	Fine.
		3	0	30,737	43	785	43		NW	1	Fine.
☉	9	9	0	30,841	39	882	36		N	1	Thick and hazy.
		3	0	30,835	42	868	42		N	1	Cloudy and hazy.
☾	10	9	0	30,833	41	795	40		N by E	1	Cloudy and hazy.
		3	0	30,804	40½	800	41		N	1	Cloudy.
♂	11	9	0	30,732	38	758	36		NE	1	Cloudy.
		2	0	30,721	42	—	42		N	1	Rain.
☿	12	8	30	30,683	34	966	38		NW	1	Foggy.
		3	0	30,631	39	882	39		—	1	Cloudy.
♃	13	9	0	30,558	36	807	36		NW	1	Cloudy and dark.
		3	0	30,481	38	849	38½		W	1	Cloudy.
♀	14	9	0	30,370	39	941	38		W	1	Cloudy.
		3	0	30,308	43	785	43		W	1	Cloudy.
♄	15	9	0	30,237	41	849	41		W	1	Cloudy.
		3	30	30,153	41	822	42		S	1	Cloudy.
☉	16	9	0	29,802	43	895	36		SW	1	Cloudy.
		3	0	29,658	44	868	43		S	1	Rain.

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for January, 1825.

1825 January.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☾	17	9	0	29,832	36	1000	36	0.012	NW	1	Fine.
		3	0	29,928	42	763	42		W	1	Fine.
♂	18	9	0	29,416	44	890	37		SW to S b W		Rain ; a violent gale.
		3	0	29,321	46	796	46	0.130	W	2	Fine.
☿	19	9	0	29,459	34	953	33		W	1	Fine.
		3	0	29,461	39	794	48	0.039	W	2	Fine.
♃	20	9	0	29,583	39	941	36		NW	2	Cloudy.
		3	0	29,598	42	790	43		N	1	Fine.
♀	21	9	0	29,733	40	829	39		N	2	Cloudy.
		3	0	29,787	40	749	42	0.003	N	2	Showery.
♄	22	9	0	29,895	36½	984	36		NE	1	Cloudy.
		2	30	30,002	46	750	46		NNE	1	Cloudy.
☉	23	9	0	30,167	36	968	35		NNE	1	Cloudy.
		3	0	30,147	40	711	41		NE	1	Cloudy.
☾	24	9	0	30,078	41	767	40		N	1	Cloudy.
		3	0	29,965	39	794	39		NW	1	Cloudy.
♂	25	9	0	29,627	30	941	35	0.092	NNW	1	Cloudy.
		3	0	29,782	40	714	40		NW	1,2	Fine.
☿	26	9	0	30,028	33	—	30	0.005	W	1	Cloudy.
		3	0	29,982	39	824	39		SW	1	Rain.
♃	27	9	0	30,040	45	824	38	0.010	W	2	Cloudy and hazy.
		3	0	30,029	50½	820	51		SW	2	Cloudy.
♀	28	8	30	30,545	37½	844	37		N	2	Fine.
		3	0	—	—	—	43		—	—	—
♄	29	9	0	30,720	32	963	32		WNW	—	—
		3	0	30,657	40	886	40		W by N	1	Fine.
☉	30	9	0	30,488	38	788	33		W	1	Cloudy.
		3	0	30,405	45	777	45		W	2	Cloudy.
☾	31	9	0	30,479	45	929	44		W	2	Cloudy.
		3	0	30,454	49	907	49		W	1	Cloudy.

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for February, 1825.

1825 February.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	o	o	o	Inches.	Points.	Str.	
♂	1	9	0	30,089	47	769	46		W	1	Cloudy.
		3	0	30,015	47	835	48		W by N	2	Rain.
☿	2	8	30	30,403	34	785	33		W	1	Fine.
		3	0	30,201	43	734	43		W by N	1	Cloudy.
♃	3	9	0	29,486	51	821	42		W	2,3	Rain.
		3	0	29,521	46	614	48	0.012	W	1	Cloudy.
♀	4	9	0	29,632	32	870	30½		N	1	Snow.
		3	0	29,700	34	759	34		W	1	Fine.
♄	5	9	0	29,681	30	730	26		W	1	Fine.
		3	0	29,678	35	758	35		W by N		—
☉	6	9	0	29,962	32	759	31		N by E	1	Fine.
		3	0	30,132	39	735	39		N by E	1	Fine.
☽	7	9	0	30,172	36	839	31½		S	2	Cloudy.
		3	0	29,997	41	849	41		SW	2	Cloudy.
♂	8	9	0	29,978	38½	909	38	0.279	W	1	Cloudy and hazy.
		3	0	29,989	46	796	46		W	1	Fine.
☿	9	9	0	30,326	36	903	34	0.008	W	1	Fine, rather hazy.
		3	0	30,362	44	781	44		W	1	Fine, some thin clouds.
♃	10	9	0	30,466	37	969	36		W	1	Hazy.
		3	0	30,020	48	777	48		W	1	Cloudy.
♀	11	9	0	30,553	40	857	37		W	1	Fine, rather hazy.
		3	0	30,527	47	703	47		W by S		—
♄	12	9	0	30,555	38	849	35		W	1	Hazy.
		3	0	30,525	43	709	44		W	1	Hazy and cloudy.
☉	13	9	0	30,564	33	929	31		SW	1	Thick fog.
		3	0	30,527	39	971	39		W	1	Cloudy and hazy.
☽	14	9	0	30,430	36	903	36½		W	1	Thick fog.
		3	0	30,345	39	882	39		SW	1	Cloudy and hazy.

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for February, 1825.

1825 February.	Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Six's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points	Str.	
♂ 15	9	0	30,139	38	970	37		E	1,2	Cloudy and thick haze.
	3	0	30,061	41	959	41		E	2	Cloudy and hazy.
☿ 16	9	0	29,998	40	943	39		W	1	Cloudy.
	3	0	30,003	45	894	45		SW	1	Cloudy.
♃ 17	9	0	30,024	44	927	39	0.005	SW	1	Cloudy.
	3	0	29,950	47	901	47		SSW	1	Cloudy.
♀ 18	9	0	29,972	45	929	45		SSE	1	Cloudy.
	3	0	30,059	47	934	47		S	1	Rain.
♄ 19	9	0	30,234	45	894	44	0.010	ESE	1	Cloudy and hazy.
	3	0	30,206	50	820	50		SE	1	Cloudy.
☉ 20	9	0	30,275	45	859	44		W	1	Cloudy.
	3	0	30,305	51	821	51		W	1	Cloudy.
☾ 21	9	0	30,472	40	943	40		——	—	Thick fog.
	3	0	30,437	47	659	47		W	1	Fine, though hazy.
♂ 22	9	0	30,456	40	915	37½		NE	1	Thick fog.
	3	0	——	45	——	45		——	—	——
☿ 23	9	0	30,197	40	866	39		ESE	1	Cloudy.
	3	0	30,149	42	711	42		E	1	Cloudy.
♃ 24	9	0	30,244	38	788	35½		NE	1	Cloudy.
	3	0	30,265	39	765	42		E	1	Cloudy and dark.
♀ 25	9	0	30,402	38	788	36		ESE	1,2	Cloudy.
	3	0	30,374	40	743	40		NE	1,2	Cloudy.
♄ 26	9	0	30,266	36½	871	35		E	1	Cloudy.
	3	0	30,177	38	849	40		ESE	1	Cloudy.
☉ 27	9	0	29,784	37	938	36		SSE	2	Rain.
	3	0	29,583	43	962	44		S	1	Cloudy.
☾ 28	9	0	29,528	36	806	34	0.078	N	1	Cloudy.
	3	0	29,603	40	629	42		N	1	Fine.

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for March, 1825.

1825 March.	Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♂ 1	9	0	29,735	37	875	33		NW	1	Fine.
	3	0	29,563	45	753	46		SSE	2	Cloudy.
☿ 2	9	0	29,209	41	932	36	0.152	W	2	Rain.
	3	0	29,195	46	727	46		W	1,2	Cloudy.
♃ 3	9	0	29,429	37	906	34	0.378	W	1	Fine.
	3	0	29,435	43	785	44½		S	1	Fine.
♀ 4	9	0	29,627	38	879	35		NW	2	Fine.
	3	0	29,766	42	842	42		N	1	Fine.
♄ 5	9	0	30,221	36	806	31		NW	1	Fine.
	3	0	30,254	43	538	44		NW	1	Cloudy.
☉ 6	9	0	30,168	40	857	36		NW	1	Cloudy.
	3	0	30,032	43	709	44		SSW.	1	Cloudy.
☾ 7	9	0	29,661	41	904	38	0.013	SE	1	Cloudy.
	3	0	29,684	45	859	45		S by E	1	Cloudy.
♂ 8	9	0	30,213	40	943	39		W	1,2	Cloudy.
	4	0	30,248	46	796	46½	—	SSW	1	Fine.
☿ 9	9	0	30,240	46	909	42		S	1	Rain.
	3	0	30,231	52	823	52	0.026	W by S	1	Cloudy.
♃ 10	9	0	30,247	50	910	49		W	1	Cloudy and dark.
	3	0	30,221	46	966	46		W by N	1	Dark and cloudy.
♀ 11	9	0	30,146	49	815	48		W	1	Rain.
	3	0	—	—	—	—		—	—	—
♄ 12	9	0	30,149	47	835	41		W	1	Fine.
	3	0	—	—	—	50		—	—	—
☉ 13	9	0	30,134	43	785	41½	0.010	NNE	1	Cloudy.
	3	0	30,046	43	886	45		E	1	Dark and rain.
☾ 14	9	0	30,104	35	658	33	0.024	E by S	1	Rain and sleet.
	3	0	30,090	36	734	36½		E by S	2	Cloudy.
♄ 15	9	0	30,174	34	862	33		E	1	Snow.
	3	0	30,223	37	570	38		E	1,2	Cloudy.
☿ 16	9	0	30,238	36	710	32		N	2	Cloudy.
	3	0	30,152	39	647	40		N	2	Cloudy.

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for March, 1825.

1825 March.		Time.		Barometer corrected.	Ther. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♃	17	9	0	30,466	31	630	28		E	1	Fine.
		3	0	30,459	37	449	37		S	1	Fine.
♀	18	9	0	—	—	—	29		—	1	Fine.
		3	0	30,557	41	541	41		SE	1	Fine.
♄	19	9	0	30,600	36	758	30		E	1	Fine, with thin clouds.
		3	0	30,548	45	588	45		SE	1	Fine.
☉	20	9	0	30,536	36	839	32		NE	1	Fine.
		3	0	30,606	48	452	48		E	1	Fine.
☾	21	9	0	30,579	40	857	34		NE	1	Cloudy.
		3	0	30,478	45	753	47		E	1	Fine.
♂	22	9	0	30,327	38	879	37		N	1	Cloudy.
		3	0	30,233	40	914	40		E	1,2	Cloudy and dark.
☿	23	9	0	30,045	42	737	37		E	2	Fine.
		3	0	29,941	44	707	45		E	2	Fine.
♃	24	9	0	30,048	39	882	33		E	1	Fine.
		3	0	29,945	47	967	47½		SE	1,2	Fine.
♀	25	9	0	29,918	42	737	37		SSE	1	Fine, rather hazy.
		3	0	29,790	48	638	49		E	1	Fine.
♄	26	9	0	30,006	45	729	38		SE	1	Fine.
		3	0	30,020	52	486	53		E by S	1	Fine.
☉	27	9	0	30,041	41	740	36		NE	1	Fine.
		3	0	29,973	51	618	51½		E	1	Fine.
☾	28	9	0	30,033	45	777	30		NE	1	Hazy and cloudy.
		3	0	30,005	50	760	52		SE	1	Cloudy.
♂	29	9	0	29,961	41	904	39		E	1	Thick, dark and hazy.
		3	0	29,907	45	824	45		E	1	Thick and cloudy.
☿	30	9	0	30,018	41	1000	41		E	1	Cloudy and hazy.
		3	0	30,046	44	890	44		E	1	Cloudy, hazy and dark.
♃	31	9	0	30,256	44	683	37		E	2	Fine.
		3	0	30,258	49	598	50		E	2	Fine.

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for April, 1825.

April. 1825		Time.		Barometer corrected.	Therm. without.	Degree of moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♀	1	9	0	30,441	43	633	38		E	1	Fine, rather hazy.
		3	0	30,414	50	—	50		E	1	Fine.
♄	2	9	0	30,465	42	619	34		N	1	Hazy.
		3	0	30,390	54	687	55		E	1	Fine.
☉	3	9	0	30,339	44	807	39		SW	1	Cloudy and hazy.
		3	0	30,248	59	314	59		NNW	1	Fine.
☾	4	9	0	30,233	50	640	41		N	1	Hazy.
		3	0	30,201	61	347	61		N	1	Fine.
♂	5	9	0	30,263	48	745	42		E	1	Fine, rather hazy.
		3	0	30,238	56	691	57		E	2	Fine.
☿	6	9	0	30,336	46	773	40		E	1	Cloudy and hazy.
		3	0	30,312	53	—	53		E	1	Fine.
♃	7	9	0	30,344	40	660	37		E	1	Fine.
		3	0	30,325	53	712	55		E	1	Fine.
♀	8	9	0	30,373	47	769	42		E	1	Fine.
		3	0	30,308	54	687	55		E	1	Fine.
♄	9	9	0	30,231	41½	904	38		N	1	Hazy.
		3	0	30,160	59	626	59		E	1	Hazy.
☉	10	9	0	30,255	52	738	46		NE	1	Fine, rather hazy.
		3	0	30,202	61	589	61		E	1	Fine, rather hazy.
☾	11	9	0	30,102	56	691	47		W	1	Fine thin clouds, rather hazy.
		3	0	—	—	—	—		—	—	——
♂	12	9	0	30,009	55	714	46½	0.007	W	2	Cloudy.
		3	0	30,027	60	522	60		S	2	Cloudy.
☿	13	9	0	30,003	51	573	47		W	1,2	Rain.
		3	0	30,081	53	766	53		N	2	Cloudy.
♃	14	9	0	30,128	57	740	47.		W	1	Cloudy.
		3	0	30,046	62	613	63		W	1	Cloudy.
♀	15	9	0	30,138	52	820	47		W	1	Fine.
		3	0	30,117	63	611	64		W	2	Fine.

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for April, 1825.

1825 April.	Time.		Barometer corrected.	Therm. without.	Degree of moisture by Daniell's Hygrom.	Six's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♄ 16	9	0	30,081	56	764	46		W	1	Fine.
	3	0	30,036	58	743	66		NW	2	Cloudy.
☉ 17	9	0	30,218	49	660	46		NE	2	Cloudy.
	3	0	30,213	53	451	63		N	1	Fine.
☾ 18	9	0	30,189	46	568	37		N	2	Fine.
	3	0	30,119	50	470	51		N	2	Cloudy.
♂ 19	9	0	30,200	45	612	35		N	1	Fine, rather hazy.
	3	0	30,175	50	485	51		W	1,2	Cloudy.
☿ 20	9	0	30,131	50	660	38		W	1	Thin clouds, and hazy.
	3	0	29,948	58	669	58		S	1	Cloudy.
♃ 21	9	0	29,952	54	844	48		W	1	Fine.
	3	0	29,837	63	553	63		W	1	Cloudy.
♀ 22	9	0	29,600	53	901	51		W	1	Rain.
	3	0	29,531	56	764	58		S	1,2	Cloudy.
♄ 23	9	0	29,432	55	840	50	0.179	W	1	Cloudy.
	3	0	29,416	63	553	64		W	1	Fine.
☉ 24	9	0	29,372	54	844	51	0.150	W	1	Cloudy.
	3	0	29,356	57	815	60		W	2	Fine, rather hazy.
☾ 25	9	0	29,660	56	764	47	0.293	E	1	Cloudy.
	3	0	29,652	63	611	63		S	1	Fine.
♂ 26	9	0	29,609	54	817	47	0.074	E	1	Cloudy; thunder at 7^{h} 45^{m} [A. M. and very dark; wind E.
	3	0	29,537	59	693	59		E	1	Fine.
☿ 27	9	0	29,273	54	791	51	0.130	E	1	Cloudy.
	3	0	29,187	61	763	59		W	1	Cloudy.
♃ 28	9	0	29,344	57	669	45	0.307	E	2	Fine.
	3	0	29,355	60	671	61		SE	2	Fine.
♀ 29	9	0	29,455	57	742	51	0.005	SE	1	Cloudy.
	3	0	29,494	63	572	64		S	1	Cloudy.
♄ 30	9	0	29,702	55	815	49		S	1	Cloudy.
	4	0	29,727	57	669	61		SSE	2	Cloudy.

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for May, 1825.

1825 May.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☉	1	9	0	29,695	54	765	48		S	2	Rain.
		3	0	29,648	60	586	60		SSE	2	Cloudy.
☾	2	9	0	29,662	56	715	47	0.608	SW	2	Cloudy.
		3	0	29,609	60	629	62		S	1,2	Cloudy.
♂	3	9	0	29,722	58	669	50	0.083	W	2	Fine, with some clouds.
		3	0	29,770	61	569	62		S	2	Fine.
☿	4	9	0	29,916	59	626	50		S	2	Cloudy.
		3	0	29,883	67	538	67		S by E		Fine.
♃	5	9	0	29,813	60	764	59	0.049	E	1	Rain.
		3	0	29,787	67	727	67		SW	1	Fine.
♀	6	9	0	29,773	65	801	60		SSE	1	Cloudy and hazy.
		3	0	29,753	71	638	71½		W	1	Cloudy.
♄	7	9	0	29,782	61	770	57		W	1	Cloudy.
		3	0	29,840	67	572	68		S	1	Fine.
☉	8	9	0	29,890	62	673	54		SSW	2	Fine.
		3	0	29,890	65	591	66		S	2	Showery.
☾	9	9	0	30,051	61	718	49	0.038	W	1	Fine.
		3	0	30,021	66	572	67		NW	1	Fine.
♂	10	9	0	30,034	61	652	50		WNW	1	Fine.
		4	0	29,991	64	629	68		N	1	Fine.
☿	11	9	0	29,980	60	739	50		E	1	Cloudy.
		3	0	29,919	61	742	65		ESE	1	Cloudy ; a shower at 2 P.M.
♃	12	9	0	29,808	55	765	47	0.099	E	1	Rain.
		3	0	29,778	56	813	58		SE	1	Rain.
♀	13	9	0	29,870	52	907	52	0.960	E	1	Rain.
		3	0	29,940	55	765	55	0.055	ENE	1,2	Cloudy.
♄	14	9	0	30,150	56	691	42		NW	1	Fine.
		3	0	30,118	59	560	61		N	1	Cloudy.
☉	15	9	0	30,195	53	595	40		N	1	Cloudy.
		3.	0	30,096	53	516	53		NE	1	Cloudy.
☾	16	9	0	30,112	55	571	45		NE	1	Cloudy.
		3	0	29,952	55	588	56		NNE	1	Cloudy.

METEOROLOGICAL JOURNAL

for May, 1825.

1825 May.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♂	17	9	0	30,131	53	712	47		NNE	1	Cloudy.
		3	0	30,146	57	599	57½		—	—	—
☿	18	9	0	30,270	53	577	44		E	1	Fine, some clouds.
		4	0	30,219	61	471	62		W	1	Cloudy.
♃	19	9	0	30,239	56	618	45		NE	1	Cloudy.
		4	0	30,206	58	624	61		SSE	1	Fine.
♀	20	9	0	30,235	59	626	42		E	1	Cloudy, rather hazy.
		3	0	30,190	63	572	64		E	1	Fine.
♄	21	9	0	30,153	57	693	44		E	1	Cloudy.
		1	30	30,121	64	621	64		ESE	1	Fine, with thin clouds.
☉	22	9	0	30,036	64	610	46		E	1	Fine.
		6	0	29,980	63	650	69		E	1	Fine.
☾	23	9	0	29,926	68	637	54		W	1	Fine, some clouds.
		3	0	29,812	74	442	76		S	1	Cloudy.
♂	24	9	0	29,738	68	593	56		W	1	Fine.
		3	30	29,695	67	635	70		W	1	Cloudy.
☿	25	9	0	29,587	65	652	54		SW	2	Fine.
		3	0	29,581	68	538	68		W	1	Fine.
♃	26	9	0	29,643	61	742	54		W	1,2	Cloudy.
		3	0	29,647	57	740	63		NE	2	Cloudy.
♀	27	9	0	29,731	53	739	46		NNE	2	Cloudy.
		3	0	29,777	53	739	56		N	2	Cloudy.
♄	28	9	0	29,872	59	560	44		W	1	Fine.
		3	0	29,818	55	765	61		SE	1	Cloudy.
☉	29	9	0	29,898	54	661	41½	0.156	SW	1	Cloudy.
		3	0	29,884	55	765	57		S	1	Fine.
☾	30	9	0	30,016	55	664	45	0.01[illegible]	NE	1	Fine.
		3	0	30,044	59	648	60		N	1	Cloudy.
♂	31	9	0	30,309	55	611	42	0.007	NNE	1,2	Cloudy.
		3	30	30,319	57	488	61		NE	1	Cloudy.

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for June, 1825.

1825 June.		Time.		Bar ometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☿	1	9	0	30,339	62	572	45		W	1	Fine.
		3	0	30,280	66	484	68		W	1	Cloudy.
♃	2	9	0	30,040	61½	672	53½	0.040	SSW	2	Cloudy.
		3	0	29,958	64	651	65		W	2	Cloudy.
♀	3	9	0	29,693	60	714	54	0.002	W	2	Cloudy.
		3	0	29,726	63	696	66		W	2	Cloudy.
♄	4	9	0	29,536	54	900	52		W	1	Rain.
		3	0	29,278	58	905	58		W	1	Cloudy.
☉	5	9	0	29,444	55	664	45		W	1	Cloudy.
		3	0	29,596	56	740	57	0.118	W	1	Cloudy.
☽	6	9	0	29,971	59	538	42		W	1	Fine.
		3	0	29,956	66	466	66		W	1	Fine.
♂	7	9	0	29,873	65	606	53		WSW	—	Fine.
		3	0	29,865	68	573	69		SSW	1	Cloudy.
☿	8	9	0	29,965	63	748	55		SW	1	Cloudy.
		3	0	29 938	71	578	71½		SW	1	Fine.
♃	9	9	0	30,069	64	748	52		W	1	Cloudy.
		3	0	30,092	69	596	73		W	1	Cloudy.
♀	10	9	0	30,280	71	558	54		SW	1	Fine.
		3	0	30,233	75	490	76		W	1	Fine.
♄	11	9	0	30,221	69	639	59		W	1	Cloudy and hazy.
		3	30	30,166	75	561	75½		SSE	1	Fine, clear blue sky.
☉	12	9	0	30,146	75	561	57		E	1	Fine.
		3	0	30,092	81	465	81		N	1	Fine.
☽	13	9	0	30,138	73	620	60		NE	1	Fine.
		3	0	30,131	80	449	81		N	1,2	Fine and clear.
♄	14	9	0	30,273	69	682	57		NE	2	Fine.
		3	0	30,256	76	580	76		E	2	Fine, blue sky.
☿	15	9	0	30,282	68	593	54		N	2	Fine, whitish sky.
		5	0	30,206	71	487	73		E	2	Fine.

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for June, 1825.

1825 June.	Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♃ 16	9	0	30,180	65	700	51		N	1	Fine.
	3	0	30,115	80	528	80		E	1	Fine.
♀ 17	9	0	30,176	64	774	54		N	2	Cloudy.
	3	0	30,114	70	504	77		E	1	Cloudy.
♄ 18	9	0	30,185	64	535	49		E	1	Fine.
	3	0	30,138	66	504	67		N	2	Fine.
☉ 19	9	0	30,043	64	651	48		SE	1	Cloudy.
	3	0	29,916	71	520	72		SW	1	Fine.
☾ 20	9	0	29,727	59	692	54		W	1	Cloudy.
	3	0	29,727	60	—	66	0.110	S	1	Cloudy; a shower at 1 P.M.
♂ 21	9	0	29,869	61	548	45		N	1	Fine.
	9	0	29,881	59	560	61		W	1	Cloudy.
☿ 22	9	0	30,049	60	564	45		W	1	Fine.
	3	0	30,032	68	421	69		NNW	1	Cloudy.
♃ 23	9	0	30,055	57	622	53		W	1	Cloudy and very dark.
	3	0	30,052	69	456	70		W	1	Cloudy.
♀ 24	9	0	29,991	67	497	52		SW	1	Fine.
	3	0	29,934	71	403	72		WNW	1	Fine.
♄ 25	9	0	29,771	70	457	56		S	1	Cloudy.
	3	0	29,673	66	702	73	0.016	W	2	Showery.
☉ 26	9	0	29,759	63	592	51		W	1	Cloudy.
	3	0	29,769	65	—	66		NW	—	Fine.
☾ 27	9	3	29,784	64	572	48½		W	1	Cloudy.
	3	0	29,780	60	693	66		NW	1	Cloudy.
♂ 28	9	0	29,753	62	721	53	0.125	SSW	2	Cloudy.
	3	0	29,683	63	673	67		S	2	Rain.
☿ 29	9	0	29,713	61	718	51	0.170	W	1	Cloudy.
	3	0	29,719	67	538	69		W	1	Cloudy.
♃ 30	9	0	29,697	61	718	56	0.112	W	1	Cloudy.
	3	0	29,680	67	635	69		W	1	Cloudy.

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for July, 1825.

July. 1825		Time.		Barometer corrected.	Therm. without.	Degree of moisture by Daniell's Hygrom.	Six's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♀	1	9	0	29,762	67	612	54		N	1	Fine.
		3	0	29,814	69	609	71		W	1	Cloudy.
♄	2	9	0	30,125	64	651	51	0.055	NNE	1	Fine.
		3	0	30,143	71	521	68		NW	1	Fine.
☉	3	9	0	30,165	65	936	54		E	1	Cloudy.
		3	0	30,094	72	472	72½		W	1	Fine.
☾	4	9	0	30,138	65	749	60		NE	1	Cloudy and hazy.
		3	0	—	—	—	73		—	—	—
♂	5	9	0	30,263	65	676	59		N	1	Cloudy and hazy.
		3	0	30,244	70	618	70		SW	1	Cloudy.
☿	6	9	0	30,085	64	798	59		N	1	Cloudy.
		3	0	30,062	63	800	67		N	1	Cloudy and showery.
♃	7	9	0	30,026	58	715	51		N	1	Cloudy and dark.
		3	0	29,994	62	653	64		N	1	Cloudy.
♀	8	9	0	29,996	60	850	56		N	2	Cloudy.
		3	0	29,986	65	725	66		N	1,2	Cloudy.
♄	9	9	0	30,034	57	937	54		NE	1	Cloudy and dark.
		3	0	29,974	67	612	67		NE	1	Fine.
☉	10	9	0	29,996	65	773	55		NE	1	Cloudy.
		3	0	29,940	69	556	73		S	2	Cloudy.
☾	11	9	0	29,948	72	579	58		W	1	Fine.
		3	0	29,898	74	506	76		S	1	Fine.
♂	12	8	15	29,959	70	639	60		W	1	Fine.
		3	0	—	—	—	82		W	1	Fine.
☿	13	10	15	30,054	75	525	62		W	1	Fine.
		3	0	30,029	79	512	81		W	1	Fine.
♃	14	9	0	30,115	70	660	61		W	1	Fine.
		3	0	30,044	79	479	80		SW	2	Fine.
♀	15	9	0	29,959	84	439	66		S	2	Fine.
		3	0	29,918	83	410	84		S	2	Fine.
♄	16	7	30	30,108	75	581	68		S	1,2	Fine.
		7	30	30,171	74	579	84		SE	1	Hazy.

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for July, 1825.

1825 July.	Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☉ 17	9	30	30,208	82	546	66		E	1	Fine, somewhat hazy.
	3	0	30,156	82	437	82½		E	2	Fine.
☽ 18	9	0	30,122	82	527	64		E	1	Rather hazy.
	4	0	30,081	84	548	85½		SE	2	Fine and clear.
♂ 19	9	0	30,127	85	549	78		N	1	Fine.
	3	0	30,120	87	533	89		E	1	Fine.
☿ 20	9	0	30,219	79	660	64		E	2	Fine.
	3	0	30,129	80	580	81		E	2	Fine.
♃ 21	9	0	30,164	67	727	59		E	2	Cloudy.
	3	0	30,161	70	618	72		E	2	Cloudy.
♀ 22	9	0	30,135	66	537	52		NNE	1	Fine.
	3	0	30,023	71	472	72		E	1	Fine.
♄ 23	9	0	29,970	68	—	69		E	1	Fine.
	3	0	29,937	70	523	71		E	1	Fine.
☉ 24	9	0	30,041	65	676	64		N	1	Cloudy.
	3	0	29,999	68	521	68		N	1	Cloudy.
☽ 25	9	0	—	—	—	—		—	—	—
	3	0	30,228	70	410	71		N	1	Cloudy.
♂ 26	9	0	30,243	64	651	55		N	1,2	Cloudy.
	3	0	30,221	67	538	69		N	1	Fine.
☿ 27	9	0	30,226	63	673	56		E	1	Cloudy.
	3	0	30,165	70	538	74		E	1,2	Fine.
♃ 28	9	30	30,129	73	429	55		N	1	Fine.
	3	0	30,091	73	488	77		E	1	Fine.
♀ 29	10	0	30,139	70	520	55		N	1	Fine.
	3	0	—	—	—	—		N	1	Fine.
♄ 30	9	0	30,050	74	850	73		N	1	Fine.
	3	0	30,079	73	660	69½		N	1	Fine.
☉ 31	9	0	—	—	—	—		—	—	—
	3	0	29,898	78	—	78		N		Fine.

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for August, 1825.

1825 August.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☾	1	9	0	29,945	70		60		W	1	Fine.
		3	0	29,926	84	357	84		W	1	Fine; rose to 86 after
♂	2	9	0	29,917	73	561	68		W	1	Cloudy. [3 o'clock.
		3	0	29,922	70	660	78		SW	2	Rain.
☿	3	9	0	29,943	68	660	57	0.068	SW	2	Cloudy.
		3	0	29,853	72	521	73		W	2	Fine.
♃	4	10	0	29,528	72	660	64	0.120	W	2	Cloudy; rain at 3 A. M.
		3	0	29,482	73	620	68		SW	2	Rain; a violent gale at 11¼
♀	5	9	0	29,461	66	631	55	0.184	NW	2	Cloudy. [P.M. Bar. 29.119
		3	0	29,519	73		74		SW	1,2	Fine. [Th. 69.
♄	6	9	0	29,605	72		59		SW	1	Rain.
		3	0	29,389	67	635	67		W	1	Cloudy.
☉	7	9	0	29,421	65	630	62		W	1	Cloudy.
		5	0	29,459	67½	635	67		W	1	Rather cloudy.
☾	8	9	0	29,429	68		62	0.141	W	1	Fine.
		3	0	29,621	70	556	72		W	1	Fine.
♂	9	9	0	29,660	67	612	57		W	2	Cloudy.
		3	0	29,635	70	488	71		W	1	Cloudy.
☿	10	9	0	29,678	65	676	54		W	1	Cloudy.
		3	0	29,684	66	655	69		S	1	Cloudy.
♃	11	9	0	29,947	60	793	53		NW	1	Cloudy.
		3	0	29,965	68	615	69		NNW	1	Cloudy.
♀	12	9	0	30,007	65	725	56		W	1	Fine, with thin white clouds.
		3	0	29,937	71	503	72		W	1	Cloudy.
♄	13	9	0								
		3	0								
☉	14	9	0	29,793	70	618	55		W	1	Cloudy.
		3	0								
☾	15	9	0					0.220			
		3	0	29,508	65	700	66		W	1,2	Cloudy.
♂	16	9	0	29,785	62	855	58	0.042	NW	1	Dark and cloudy.
		3	0	29,800	69	660	69		W	1	Cloudy.

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	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☿ 17	9	0	29,827	65	749	57		W	1	Fine, rather hazy.
	3	0	29,812	69	706	73		W	1	Cloudy.
♃ 18	9	30	29,950	63	774	59	0.005	N	1	Cloudy.
	3	0	30,127	67	727	69		N	1,2	Cloudy.
♀ 19	9	0	30,177	62	774	58		N	1	Cloudy.
	3	0	30,124	63	774	65		N	1	Cloudy.
♄ 20	9	0	30,311	69	575	61		S	1	Fine.
	3	0	—	—	—	—		—	—	—
☉ 21	9	0	30,330	69	596	58		N	1	Fine.
	3	0	—	—	—	—		—	—	—
☾ 22	9	0	—	—	—	—		—	—	—
	3	0	30,200	67	704	69		E	1	Fine and clear.
♂ 23	9	0	30,126	68	799	61		E	1	Fine.
	3	0	30,060	71	661	72		E	1	Fine.
☿ 24	9	0	30,082	64	774	55		E	1	Fine, rather hazy.
	3	0	30,054	72	705	73		E	1	Fine.
♃ 25	9	0	30,199	68	776	55		E	1	Cloudy.
	3	0	30,074	68	729	71		E	1	Fine.
♀ 26	9	0	30,124	66	801	54		E	1	Cloudy.
	3	0	30,105	68	704	70		SE	1	Fine.
♄ 27	9	0	30,052	60	821	60		W	1	Rain.
	3	0	29,985	61	941	61		NE	1	Rain.
☉ 28	9	0	29,948	61	971	55		SE	1	Cloudy.
	3	0	29,923	60	970	65		NW	1	Cloudy.
☾ 29	9	0	29,932	65	853	60	0.397	N	1	Rain.
	3	0	30,010	66	801	66		W	1	Rain.
♂ 30	9	0	30,077	67	777	63		SW	1	Fine, rather hazy.
	3	0	30,033	76	635	77	0.078	SW	1	Fine.
☿ 31	9	0	30,075	68	823	65		W	1	Cloudy and hazy.
	3	0	30,033	75	725	76		E	1	Cloudy.

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for September, 1825.

1825 September.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Six's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♃	1	9	0	30,071	69	752	61		NNE	1	Fine.
		3	0	30,011	70	598	77		N	1	Fine.
♀	2	9	0	30,140	68	729	62		NNE	1	Cloudy.
		3	0	30,107	72	702	74		N	1	Fine.
♄	3	9	0	30,172	61	949	61		N	1	Cloudy.
		3	0	30,045	69	639	70		——	1	Cloudy.
☉	4	9	0	29,935	61	610	56		S	1	Fine.
		3	0	29,970	66	——	74		W	1	Fine.
☾	5	12	0	30,031	60	764	58		W	1	Cloudy.
		3	0	30,010	61	718	61		N	1	Cloudy.
♂	6	9	0	29,950	62	697	59		NW	1	Fine.
		3	30	29,863	65	725	67		N	1	Cloudy.
☿	7	9	0	29,724	60	793	52		W	1	Fine, somewhat hazy.
		3	0	29,644	68	554	68		W	1	Cloudy.
♃	8	9	0	29,604	61	770	56		W	1	Fine.
		3	0	29,605	68	573	68		SW	1	Fine.
♀	9	9	0	29,687	63	673	53		W	1	Fine.
		3	0	——	—	——	—		——	—	——
♄	10	9	0	29,620	65	676	58		NW	1	Fine, rather cloudy.
		3	0	29,449	69	618	70		S	1,2	Cloudy.
☉	11	9	0	29,540	63	748	60		SSW	1	Cloudy.
		3	0	——	—	——	—		——	—	——
☾	12	9	0	29,708	64	774	58		S	1	Fine, a few clouds.
		3	0	29,818	68	682	69		SSE	2	Cloudy.
♄	13	9	0	——	—	——	—		——	—	——
		3	0	——	—	——	—		——	—	——
☿	14	9	0	29,430	61	971	58		S	1	Cloudy and hazy.
		3	0	29,346	67	753	67		N	1	Rain.
♃	15	9	0	29,532	63	855	61	0.750	W	1	Cloudy.
		3	0	29,629	66	749	67		W	1	Cloudy.

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1825 September.	Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♀ 16	9	0	29,807	64	827	56		S	1	Cloudy.
	3	0	29,788	68	752	69		S	1	Cloudy.
♄ 17	9	0	29,778	69	797	62		SW	1	Cloudy.
	3	0	29,743	68	968	73		SW	1	Rain.
☉ 18	9	0	29,784	67	880	66	—	SW	1	Cloudy.
	3	0	29,715	66	851	70		SW	1	Cloudy.
☾ 19	9	0	29,749	66	856	63	0.455	SW	1	Cloudy.
	3	0	29,764	69	729	70		SW	2	Cloudy.
♂ 20	9	0	29,752	65	700	61		SSW	1	Cloudy.
	3	0	29,737	70	799	71		S	2	Cloudy.
☿ 21	9	0	29,547	66	801	63	0.040	W	1	Cloudy.
	3	0	29,542	70	726	71		8	1	Fine.
♃ 22	9	0	29,639	64	854	59	0.052	W	1	Cloudy.
	3	0	29,774	64	629	65½		W	1	Cloudy.
♀ 23	9	0	30,008	54	817	50		W	1	Hazy.
	3	0	29,990	63	722	64		SW	1	Cloudy.
♄ 24	9	0	29,986	67	777	59		SW	1	Fine.
	3	0	30,003	67	777	70		W	1	Cloudy.
☉ 25	9	0	30,028	65	826	63		W	1	Cloudy, rather hazy.
	3	0	29,990	69	775	71		NW	1	Fine.
☾ 26	9	0	29,983	61	912	60		NE	1	Rain.
	3	0	29,902	65	801	68	0.562	W	2	Fine.
♂ 27	9	0	30,115	57	874	51		W	1	Fine.
	3	30	30,173	63	748	65		N	1	Fine.
☿ 28	9	0	30,337	52	935	49		NE	1	Cloudy and hazy.
	8	30	30,273	57	937	65		—	1	Fine.
♃ 29	9	0	30,245	57	843	51		E	1	Fine.
	3	0	30,123	59	717	61	—	E	1	Fine.
♀ 30	9	0	29,948	52	738	47		E	1	Fine.
	3	0	29,873	59	737	59½		E	1	Fine.

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1825 October.		Time. H.	M.	Barometer corrected. Inches.	Therm. without. °	Degree of moisture by Daniell's Hygrom. °	Six's Therm. °	Rain. Inches.	Winds. Points.	Str.	Weather.
♄	1	9	0	29,802	58	935	56		W	1	Rain.
		3	0	29,807	62	914	65		W	1	Fine.
☉	2	9	0	29,721	62	855	56		N	1	Rain.
		3	0	—	—	—	—		—	—	——
☽	3	12	0	29,621	60	—	59	0.618	E	1	Rain.
		3	0	29,592	64	881	65		S	1	Fine.
♂	4	9	0	29,309	63	855	58	0.040	SW	1	Cloudy.
		3	0	29,895	66	749	68		SSW	1	Fine.
☿	5	9	0	30,074	61	880	56		S	1	Cloudy.
		3	0	30,060	64	827	65½		S	1	Fine.
♃	6	9	0	30,013	63	826	60	0.055	SSE	2	Cloudy, a gale in the night.
		3	0	29,925	65	773	67		SSE	1	Cloudy.
♀	7	9	0	29,487	59	847	57	0.280	W&SW	3	Cloudy, blowed hard all night.
		3	0	29,830	61	652	62		W	2	Cloudy.
♄	8	9	0	29,989	60	693	59		NE	1	Fine.
		3	0	29,859	62	721	63		N	1	Fine.
☉	9	9	0	29,884	63	696	51		W	1	Fine.
		3	0	29,934	60	770	62		W	1	Cloudy.
☾	10	9	0	30,184	62	828	58	0.485	W	2	Cloudy.
		3	0	30,215	62	855	63		SW	2	Cloudy.
♂	11	9	0	30,339	61	912	59		W	1	Fine.
		3	0	30,298	63	826	64		W	1	Cloudy.
☿	12	9	0	30,125	58	935	53		E	1	Cloudy.
		3	0	30,024	63	826	65		S	1	Fine.
♃	13	9	0	30,131	59	847	57	0.005	W	1	Cloudy.
		3	0	30,170	63	722	63		NW	1	Cloudy.
♀	14	9	0	30,136	55	870	51		W	1	Fog, a very thick fog at 7 A. M.
		3	0	—	—	—	63		—	—	——
♄	15	9	0	30,439	51	937	47		W	1	Fine, rather hazy.
		3	0	30,463	56	841	57		NW	1	Fine.
☉	16	9	0	30,492	51	879	44		W	1	Cloudy.
		3	0	30,418	59	818	60		SW	1	Cloudy.

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1825 October.	Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☾ 17	9	0	30,218	52	879	47	0.005	W	1	Cloudy.
	3	0	30,051	56	791	56		W	1	Rain.
♂ 18	9	0	30,108	47	934	42		W	1	Cloudy.
	3	0	29,675	50	760	50	0.020	W	2	Rain.
☿ 19	9	0	*29,347	51	763	48½	0.069	W	1	Cloudy.
	3	0	29,131	52	794	54		W	1,2	Rain.
♃ 20	9	0	29,048	41	877	36	0.272	N	1	Fine.
	3	0	29,148	42	618	43		N	2	Fine.
♀ 21	9	0	29,388	40	743	36		NNW	2	Fine.
	3	0	29,476	44	659	44½		NW	2	Fine.
♄ 22	9	0	29,858	46	830	39		NNE	2	Cloudy and showery.
	3	0	29,971	49	784	50		N	1,2	Fine.
☉ 23	9	0	30,097	38	879	35		NW	1	Fine.
	3	0	30,071	45	753	45		W	1	Fine, thick fog at 12 at noon.
☾ 24	9	0	29,925	49	814	44	0.021	W	1	Cloudy and hazy.
	3	0	29,827	54	713	55		W	1	Cloudy.
♂ 25	9	0	29,904	46	830	41	0.015	W	1,2	Cloudy and hazy.
	3	0	29,900	46	773	47	0.028	NW	2	Cloudy,
☿ 26	9	0	30,074	37	906	33		W	1,2	Hazy.
	3	0	29,989	45	659	45½		W	1	Cloudy and dark.
♃ 27	9	0	30,071	43	924	42		N by E	1	Fine, rather hazy.
	3	0	30,022	47	681	47		NW	2	Cloudy.
♀ 28	9	0	29,981	51	821	45		W	1	Cloudy.
	3	0	29,976	57	677	58		NW	1	Cloudy.
♄ 29	9	0	30,017	55	840	52		W	1	Cloudy.
	3	0	29,919	57	740	50		SW	1	Fine.
☉ 30	9	0	29,961	52	794	43	0.085	W	1	Cloudy.
	3	0	29,856	56	841	56		W	1	Cloudy.
☾ 31	9	0	29,989	48	808	44½		W	1	Fine.
	3	0	29,982	52	794	53		W	1	Cloudy.

* Mercury adheres to one side of the tube, and with difficulty disengaged from it.

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1825 November.	Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♂ 1	9	0	29,933	51	879	46		W	1	Cloudy, rain in the night.
	3	0	29,754	57	815	58		W	1	Cloudy.
☿ 2	9	0	29,794	48	777	46	0.050	W	1	Cloudy.
	3	0	29,705	52	738	52		NW	1	Cloudy.
♃ 3	9	0	28,947	54	713	49½		SW	3	Fine.
	3	0	28,952	48	840	54		W	1	Rain.
♀ 4	9	0	29,525	42	895	39	0.081	NW	1	Fine.
	3	0	29,667	46	—	48		NW	1	Cloudy.
♄ 5	9	0	29,954	38	879	35		W	1	Fine.
	3	0	——	—	—	—		—	—	——
☉ 6	9	0	29,156	56	789	38	0.030	W	2	Cloudy.
	3	0	29,203	51	705	57		W	1	Cloudy.
☾ 7	9	0	29,231	40	771	39	0.006	W	1	Cloudy and hazy.
	3	0	29,236	43	734	44		W	1	Cloudy.
♂ 8	9	0	29,374	38	848	33½		SW	1	Cloudy.
	3	0	——	—	—	—		—	—	——
☿ 9	9	0	29,066	42	874	38	0.400	W	1	Fine.
	3	0	29,140	43	835	45		W	1,2	Fine.
♃ 10	9	0	28,740	42	961	38	0.288	SSW	3	Rain.
	3	0	28,732	42	—	43		N	3	Rain.
♀ 11	9	0	29,367	39	824	37	0.212	N	1	Rain.
	3	0	29,427	42	868	42		NE	2	Cloudy.
♄ 12	9	0	29,806	36	685	35	0.008	N	1,2	Cloudy.
	3	0	29,845	43	557	44		N	1	Fine.
☉ 13	8	30	29,950	31	875	29		NW	1	Hazy.
	3	0	29,899	37	758	37		W	1	Fine, rather hazy.
☾ 14	9	0	29,870	40	857	35		N	1	Foggy.
	3	0	29,870	44	1000	46		NNW	1	Fine.
♂ 15	9	0	30,094	40	886	35½		N	1	Fine.
	3	0	30,090	42	790	43		N	1	Fine.

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for November, 1825.

1825 November.	Time.		Barometer corrected.	Therm. without.	Degree of moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
	H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
☿ 16	9	0	30,181	35	867	32		NW	1	Foggy.
	3	0	30,171	40	857	40		W	1	Cloudy and dark.
♃ 17	9	0	30,100	40	943	38				Foggy and cloudy.
	3	0	29,961	46	864	46		W	2	Cloudy.
♀ 18	9	0	29,977	39	971	38		W by N	1	Fine.
	3	0	29,809	48	936	48		S	1,2	Rain.
♄ 19	9	0	29,886	41	816	41	0.102	W	1	Cloudy.
	3	0	29,886	46	796	46		W	1	Fine.
☉ 20	9	0	30,172	36	968	34		W	1	Fine, somewhat hazy.
	3	30	30,160	43	886	43		SW	1	Cloudy and hazy.
☾ 21	9	0	29,784	53	820	43		W	3	Cloudy.
	3	0	29,739	53	793	48		W	2	Rain.
♂ 22	9	0	29,863	39	912	38		W	1	Fine.
	3	0	29,963	44	781	45		N	2	Cloudy.
☿ 23	9	0	30,283	36	871	34		W	1	Hazy and cloudy.
	3	0	30,210	44	805	44		W by N	1	Fine.
♃ 24	9	0	30,161	50	910	44	0.128	N	1	Hazy.
	3	0	30,067	51	908	51		SW	2	Rain.
♀ 25	9	0	30,234	43	962	42	0.118	W	1	Fine.
	3	0	30,277	46	773	47		N by W	1	Fine.
♄ 26	9	0	30,072	45	824	38	0.090	W	1	Cloudy.
	3	0	29,795	50	850	50		W	1	Rain.
☉ 27	9	0	29,949	41	767	40	0.060	W	1	Fine.
	3	0	29,881	45	753	45		W	1	Cloudy.
☾ 28	9	0	29,272	51	908	38	0.035	W	2	Rain.
	3	0	29,151	52	823	53			1	Cloudy.
♂ 29	9	0	28,956	44	963	43		S	1	Fine.
	3	0	29,324	48	968	49		SW	2	Rain.
☿ 30	9	0	29,389	40	959	40	0.048	W	3	Rain; strong N.W. gale in [the night.
	3	0	29,568	41	822	40		W	1	Fine.

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1825 December.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°	Inches.	Points.	Str.	
♃	1	9	0	29,590	33	929	31		S	1	Foggy.
		3	0	29,448	36	1000	38		W	1	Fine.
♀	2	9	0	28,925	45	824	35	0.283	W	2	Rain.
		3	0	28,996	44	854	46	0.050	W	2	Cloudy.
♄	3	9	0	29,175	41	904	40	0.005	SW	2	Fine.
		3	0	29,234	43	835	35		SW	1	Cloudy.
☉	4	9	0	29,126	40	943	39½	0.042	N	1	Rain.
		3	0	29,231	41	932	41		NNE	1	Cloudy.
☾	5	9	0	29,341	35	967	33½	0.230	N	2	Rain.
		3	0	29,248	42	961	43		N	1	Cloudy.
♂	6	9	0	29,371	47	835	42	0.315	S	1,2	Cloudy.
		3	0	29,341	48	904	49	0.010	SSE	1	Cloudy.
☿	7	9	0	29,197	45	894	42		E	1	Cloudy.
		3	0	29,210	50	790	51		E	1	Cloudy.
♃	8	9	0	29,343	45	894	44		E	2	Cloudy, dark and hazy.
		3	0	29,303	44	854	45		NNE	2	Dark and cloudy.
♀	9	9	0	29,365	45	776	44		E	1	Dark and cloudy.
		3	0	29,434	45	859	45		NE	1	Cloudy and dark.
♄	10	9	0	29,617	44	829	44	0.020	W	1	Cloudy and dark.
		3	0	29,656	45	859	45		W	1	Cloudy.
☉	11	9	0	29,735	43	886	43	0.035	W	1	Cloudy.
		3	0	29,756	45	859	47		W	1	Cloudy.
☾	12	9	0	29,845	41	904	40			1	Very thick fog.
		3	0	29,833	42	1000	42			1	Very thick fog.
♂	13	9	0	29,777	36	936	36	0.005	S	1	Thick fog.
		3	0	29,690	46	830	46		S	1,2	Cloudy and foggy.
☿	14	9	0	29,314	47	868	46	0.185	E	3	Rain.
		3	0	29,265	52	850	52		SE	1	Cloudy.
♃	15	9	0	29,530	38	879	37	0.120	W by N	1	Fine.
		3	0	29,691	41	849	42		NW	1	Fine.
♀	16	9	0	29,660	51	763	40	0.015	W	1	Fine.
		3	0	29,773	51	792	53		W	1	Fine.

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1825 December.		Time.		Barometer corrected.	Therm. without.	Degree of Moisture by Daniell's Hygrom.	Sixe's Therm.	Rain.	Winds.		Remarks.
		H.	M.	Inches.	°	°		Inches.	Points.	Str.	
♄	17	9	0	29,681	51	850	49		SSW	2	Rain.
		3	0	29,707	51	850	53½		W	1	Cloudy.
☉	18	9	0	29,640	48	872	44	0.155	S	1,2	Fine.
		3	30	29,560	51	821	51		S	1	Cloudy.
☾	19	9	0	29,317	49	938	49	0.013	S	2	Rain.
		3	0	29,313	47	901	50		NW	2	Cloudy.
♂	20	9	0	29,402	45	894	41	0.066	E	1	Fine.
		3	0	29,404	50	—	50		E	1	Fine.
☿	21	9	0	29,404	50	820	45		E	1	Fine.
		3	0	29,427	51	821	51		E	1	Cloudy.
♃	22	9	0	29,472	47	967	47	0.080	W	1	Cloudy.
		3	0	29,563	47	967	48		W	1	Cloudy.
♀	23	9	0	29,829	41	932	40		W	1	Cloudy.
		3	0	29,775	45	929	45		SW	1	Cloudy.
♄	24	9	0	30,071	37	938	36	0.098	NW	1	Fine.
		7	0	30,094	39	971	43				Fine.
☉	25	9	30	29,710	49	876	39	0.010	SW	2	Rain.
		3	30	29,768	47	703	50		NW	2	Fine.
☾	26	9	0	29,908	38	879	37		W	1	Cloudy and hazy.
		3	0	29,720	43	861	43		W	1	Cloudy.
♂	27	9	0	29,841	31	760	30		W	1	Fine, rather hazy.
		3	0	29,777	33	585	33½		NW	1	Fine.
☿	28	9	0	29,646	32	815	29		NW	1	Cloudy and hazy.
		3	0	29,582	36	782	36		W	1	Cloudy.
♃	29	9	0	29,546	33	964	33		N	1	Cloudy and dark.
		3	0	29,525	35	933	36		NE	1	Cloudy and dark.
♀	30	9	0	29,582	33	929	32½		W	1	Cloudy, hazy, and dark.
		3	0	29,610	35	808	36		W	1	Cloudy.
♄	31	9	0	29,714	28	936	28		W	1	Fine, rather hazy.
		3	30	29,751	35	833	35		W	1	Fine.

1825.	Height of Barometer,* corrected.			Height of Thermometer without.			Degrees of Moisture by Daniell's Hygrometer.			Sixe's thermometer.			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.
January	30,841	29,321	30,211	54	32	40,8	1000	579	838	54	32	40,3	0,467
February	30,564	29,486	30,127	51	30	40,9	971	614	834	51	26	40,1	0,392
March	30,660	29,195	30,075	52	31	42,4	1000	449	772	53	29	40,6	0,603
April	30,465	29,187	29,975	63	45½	54,2	904	314	668	66	34	51,3	1,145
May	30,319	29,581	29,608	71	52	59,8	907	442	652	71½	40	55,7	2,073
June	30,389	29,278	29,950	81	54	65,9	905	403	604	81	42	61,0	0,693
July	30,263	29,762	30,072	87	57	71,1	937	410	607	89	51	67,4	0,055
August	30,330	29,389	29,883	84	60	67,7	971	357	503	84	53	64,5	1,255
September	30,337	29,346	29,852	72	52	64,1	971	554	753	74	47	62,8	1,859
October	30,492	29,048	29,920	66	37	54,4	937	618	807	68	33	52,7	1,998
November	30,277	28,732	29,699	57	31	44,1	1000	557	843	58	29	42,5	1,656
December	30,094	29,126	29,538	51	28	42,7	1000	585	875	53½	28	41,9	1,737
Whole year			29,909			54,0			729			51,7	13,933

* The quicksilver in the bason of the barometer is 100 feet above the level of low water spring tides at Somerset-place.

† The Rain Gage is 114 feet above the same level, and 75 feet above the surrounding ground.

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SOCIETY FOR THE ENCOURAGEMENT OF ARTS, MANUFACTURES AND COMMERCE.—Transactions of the Society instituted at London for the Encouragement of Arts, Manufactures, and Commerce, with the Premiums offered in the year 1824. Vol. XLIII. 8° *London*, 1825.	The Society for the Encouragement of Arts, Manufactures, and Commerce.
HORTICULTURAL SOCIETY.—Transactions of the Horticultural Society of London. Vol. VI. Part III. 4°.	The Horticultural Society.
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AIRY (G. B.) Mathematical Tracts on Physical Astronomy, the Figure of the Earth, &c. 8° *Cambridge.*	G. B. Airy, Esq.
ALMANAC. The Nautical Almanac and Astronomical Ephemeris for the year 1828. 8° *London.*	The Commissioners of Longitude.
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ERRATA.

Page 262, line 21, for *second*, read, *half second*.
—— 263, line 16, after *diminishing it*, insert, *they then return in a similar manner in an opposite direction*.
—— 263, line 19, instead of *before its termination*, read, *after its commencement*.
—— 268, line 5, for *not all*, read, *not at all*.
—— 274, line 10, for *became*, read, *because*.

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